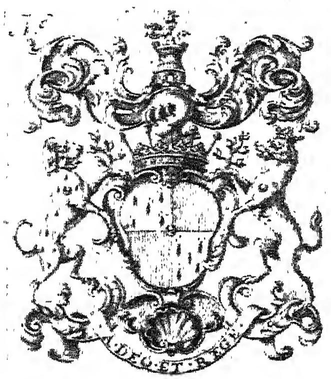
DEO·ET·REGE

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Prefent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. LIX. For the Year 1769.

LONDON:

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M.DCC.LXX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

 should

ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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In the YEAR 1769;

WITH

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PHILO

PHILOSOPHICAL TRANSACTIONS.

I. *A Letter from Mr.* J. Moult *to Dr.* Percival, *of* Manchester, *F. R. S. containing a new Manner of preparing Salep.*

SIR,

Read Jan. 12, 1769.

AS the specimen of Salep, which I left you some time ago, meets with your approbation, so far as to think it deserving to be laid before the Royal Society, I now send you my method of curing the common Orchis roots of our own country, so as perfectly to resemble what comes to us from Turky. And if the communication be of any public utility, I shall think myself sufficiently gratified for the trouble I have had in prosecuting the experiments necessary thereto.

The roots I have hitherto made uſe of, are thoſe of the *orchis morio mas foliis maculatis* of Parkinſon, the *cynoſorchis morio mas* of Gerard, and the *cynoſorchis major*, *vulgo* dog-ſtones: though, from a ſpecimen of the *orchis palmata major mas* of Gerard, which you have among the Salep, that root likewiſe appears capable of being made to anſwer the ſame purpoſes as the others. The beſt time to gather the roots is, when the ſeed is formed, and the ſtalk going to fall; for then the new bulb, of which the Salep is made, is arrived to its full ſize, and may be known from the old one, whoſe ſtrength is then ſpent by the preceding germination, by a white bud riſing from the top of it, which is the germ of the plant of the ſucceeding year. This new root, being ſeparated from the ſtalk, is to be waſhed in water, and a fine thin ſkin, that covers it, to be taken off with a ſmall bruſh; or, by dipping in hot water, it will come off with a coarſe linen cloth.

When a ſufficient quantity of the roots is thus cleaned, they are to be ſpread on a tin plate, and ſet into an oven, heated to the degree of a bread-oven, where they are to remain ſix, eight, or ten minutes; in which time they will have loſt their milky whiteneſs, and have acquired a tranſparency like that of horn, but without being diminiſhed in ſize. When they are arrived at this ſtate, they may be removed to another room to dry and harden, which will be done in a few days; or they may be finiſhed in a very ſlow heat, in a few hours. I have tried both ways with ſucceſs.

The orchis's above-mentioned grow ſpontaneouſly in this part of the country, and throughout the whole kingdom.

kingdom. They flouriſh beſt in a dry, ſandy, barren ſoil. As the method of curing this root is ſo eaſy, I hope it will encourage the cultivation of ſo nutritious a vegetable, ſo as to reduce it from its preſent high price, which confines it to people of fortune, to one ſo moderate as would bring it into common uſe, like other kinds of meal or flour; and ſo become a valuable addition to our preſent liſt of eatables, its quality of thickening water being to that of fine flour nearly as 2½ to 1, with this difference, that the jelly of Salep-powder is clear and tranſparent, whereas that of flour is turbid and white.

If this ſhould find you in the ſame ſentiments reſpecting it, I give you liberty to make uſe of it accordingly.

And am,

with all reſpect,

Your very humble ſervant,

Rochdale, Nov. 10, 1768.

J. Moult.

Received November 9, 1769.

II. *Brevis Narratio de Structura et Effectu Speculorum cauſticorum parabolicorum à defuncto* D^no^ Hoeſen Dreſdae *elaboratorum, quae nunc à* D^no^ Ehrard, *ſub Arce* Dreſdenſi *habitante, poſſidentur. Auctore* D^no^ Wolfe, *M. D.*

Read Jan. 12, 1769.

ORBIS vel rectius ſegmentum parabolicum concavum ex pluribus aſſeribus ſolidioris ligni compoſitum, et in convexa parte baculis tam a vertice divergentibus, quam tranſverſis probe colligatum et firmatum, obductum eſt in concava parte laminis aurichalceis, quarum craſſities $\frac{1}{8}$ circiter pollicis, longitudo pedum quatuor cum dimidio, latitudo vero pedum duorum cum dimidio. Hæ laminæ tanto ſtudio inter ſe ſunt conferruminatæ, ut linea jungens duas tales laminas vix appareat: ſplendore vero tali pollent, qualis ſumma cum cura poliendo conciliari aurichalco poteſt. Suſpenditur et movetur tale ſpeculum inter duo brachia ſemicirculi lignei, mobilis circa axim ligneum verticalem, inſiſtentem pedibus tribus, eorumque rotulis. Pondus pro ratione magnitudinis valde exiguum, ut unica manu in omnem ſitum facile dirigi poſſit. Anteriorem

riorem ſpeculi partem ſubtendit arcus ferreus ½ pollicem craſſus, verſus medium, hoc eſt in ipſo loco foci uſtorii, in annulum efformatus, qui annulus ex utraque parte furcam ferream gerit, quibus vaſculum ferreum corpus examinandum continens imponitur. Poſſidet D^nus^ Erhard ſex talia ſpecula, quorum quatuor magis perfecta ſequentes habent dimenſiones.

Num.	Perimeter.		Diameter ſeu Ordinata.		Profunditas ſeu Abſciſſa.		Diſtantia foci a vertice.	
	Ped.	Pollic.	Ped.	Poll.	Ped.	Poll.	Ped.	Poll.
1. *	29	4	9	7	1	4	4	0
2.	21	0	6	8	0	10¼	3	1
3.	16	4	5	1	0	10½	1	10
4.	13	2½	4	2	0	7	1	9

Pes Dreſdenſis ad Londinenſem fere ut 13 : 14. Primum ſpeculum pro faciliori tranſportatione in duas partes diviſum eſt. Curvaturam horum ſpeculorum ſatis exactam eſſe, patet ex eo, quod diameter foci priorum quatuor dimidium pollicem non excedat. Utinam color et politura ſpeculorum metallicorum iis conciliari poſſet! Effectus horum, urendo, calcinando, fundendo, vitrificando, multum ſuperat quodcunque de hujus generis experimentis innotuit. Duriſſimi lapides vix pauca ſecunda temporis reſiſtunt: metalla cito perforantur: vegetabilia nictu oculi comburuntur in cinerem et vitrum, æque ac oſſa animalium in calcem et vitrum.

Lubet hic referre pauca experimenta a D^no^ D^re^ Hoffman, nuper inſtituta, cum ſpeculo quod tertium in ordine.

1. Minera argenti nativi in lapide talcoſo viridi, fundebatur tempore unius ſecundi, abſque ullo fumo.

Poſt unius minuti primi fuſionem inveniebantur permulta argenti granula lapidi in vitrum nigrum mutato adhærentia.

2. Minera cupri pyriticoſa Salfendenſis cum malachite interſperſo fluebat, uti præcedens, in momento: et tribus ſecundis vix elapſis, guttæ rubræ depluebant, quæ frigefactæ cuprum fragile erant; lapis vero nunc erat vitrum ex rubro nigrum.

3. Minera ſtanni polyedra Altenburgenſis in momento fluens, poſt unius minuti fuſionem, inveniebatur multa ſtanni grana dimiſiſſe; ipſe vero lapis quarzoſus in vitrum nigrum mutatus erat.

4. Galena plumbi teſſulis majoribus in nictu oculi fundebatur cum fumo denſo, et poſt tria ſecunda temporis plumbum bonum malleabile depluebat.

5. Hæmatites niger ſtriatus quarto temporis ſecundo incipiebat fundi, ſine omni fumo, et poſt duo minuta prima ablato, perfecti ferri aliquid adhærere cernebatur.

6. Aſbeſtus Hungaricus tempore trium ſecundorum in vitrum abibat ex flavo viride.

7. Glacies Mariæ Eiſlebenſis in momento calcinabatur; poſt 4 vel 5 minuta prima in loco foci perforabatur, atque in vitrum ex viridi flavum vertebatur, ſimile illi quod ex aſbeſto provenit.

8. Schiſtus vulgaris niger tegularis tempore duorum ſecundorum abibat in vitrum nigrum.

9. Marmor nigrum in loco, quem focus attingebat, tempore aliquot ſecundorum calcinabatur, et poſt unum minutum primum hinc inde fundi incipiebat.

10. Fruſtum lapidis ſabuloſi martialis tempore trium ſecundorum in vitrum nigrum fundebatur.

11. Num-

11. Nummus aureus Hiſpanicus (*a piſtole*) fundebatur intra duo ſecunda, fuſus niger apparebat.

12. Florenus Saxonicus (*half a crown*) in momento fundi incipiebat, et intra tria ſecunda jam perforatus erat.

13. Clavus ferreus ex rota, tempore trium ſecundorum fluebat, et tempore quinque jam tres ferri guttæ piſi majoris magnitudine confluxerant in unam, inſimulque vitri viridis parum productum erat.

14. Si ignis cum flamma intra focum et ſpeculum ponatur, tempore nocturno obſcuro, magna circumjacens plaga ita illuminatur, ut etiam in horologio turris diſtantis numerus horam indicans diſcernatur.

15. Si ignis prunarum bene accenſarum ponatur directe ante ſpeculum, licet notabili diſtantia, calor in foco ſatis intenſus percipitur, ut etiam candela accendi, vel aliæ res inflammabiles comburi poſſint.

16. Si vero prunæ accenſæ in ipſo foco ponantur, et radii inde egreſſi a ſpeculo reflexi ab alio ſpeculo in diſtantia ſatis notabili excipiantur, poterunt res inflammabiles in hujus ſecundi ſpeculi foco poſitæ accendi.

17. Experimenta duo ultima non ſolum ſuccedunt cum ipſis prunis accenſis, ſed etiam, cum fornace fortiter calefacto, in foco ſpeculi poſito, vel juxta experimentum 15^{tum} ante eundem, diſtantiis nempe probe ſelectis.

18. In camera obſcura imagines objectorum ab his ſpeculis reflexæ diſtincte et cum propriis coloribus repræſentantur. Præterea cum microſcopio ſolari hæc ſpecula utiliter combinari poſſunt.

19. Si duo ſpecula in notabili diſtantia, uti 50 paſſuum, directe ſibi invicem opponantur, et in

foco

foco unius verba quædam voce admodum depressa proferantur, vel horologium minus marsupiale ibi ponatur, poterunt ista verba vel horologii vibrationes in foco alterius speculi distincte audiri.

Qu. Specula Archimedea erantne diversa ab his? Certe parabola, cujus parameter bis mille pedum, non difficulter describitur. Forte etiam radii a tali speculo reflexi, post focum a lente excipi, et situ parallelo ad omnem distantiam mitti possent, habita nempe ratione fusibilitatis vitri.

Sept. 2, 1768.

Received December 8, 1768.

III. *An extraordinary Cafe of three Pins fwallowed by a Girl, and difcharged at her Shoulder. In a Letter to* Frank Nicholls, *M. D. F. R. S. from Dr.* Lyfons, *of* Gloucefter.

To Charles Morton, M. D. Sec. R. S.

Epfom, Nov. 25, 1768.

DEAR SIR,

Read January 26, 1769.

INCLOSED I fend you a moft extraordinary cafe, which is tranfmitted to me by Dr. Lyfons, a gentleman of great learning and credit, and phyfician to the Gloucefter hofpital. It feems to be exactly drawn, and the Doctor's veracity may be depended on. I think it well worth preferving in the Memoirs of the Society; and believe that the Council will have the fame opinion of it as,

SIR,

Your very humble fervant,

Fran. Nicholls.

SIR,

UPON my mentioning the caſe of a girl who ſwallowed three pins, which were afterwards diſcharged at her ſhoulder, you thought it might be proper for the Philoſophical Tranſactions, and deſired me to ſend it you. I have drawn it from notes taken during my attendance upon her, with as much accuracy as poſſible, and it is as follows.

Eleanor Kaylock, a robuſt, ſtrong girl, aged twenty-two, was admitted a patient in the Glouceſter infirmary, May 29, 1766, for a pain in her ſide proceeding from pins ſwallowed three quarters of a year before. The occaſion of the accident was thus. Being employed in the buſineſs of a kitchen, as ſhe was ſcumming the pot (her mouth being open, and three pins in it), ſhe received a quantity of the vapour, which obliged her to ſwallow, and the pins at the ſame time paſſed into the *œſophagus*, where they remained for eight weeks, notwithſtanding various methods were uſed for their removal; but they were at laſt forced down by the whalebone inſtrument uſed by ſurgeons for that purpoſe.

Whilſt the pins were in her throat, the parts became inflamed, and ſwoln, which occaſioned an hoarſeneſs, attended with great pain, and difficulty of breathing: being alſo capable of receiving but very little nouriſhment, and that liquids, ſhe was reduced to ſo weak a ſtate as not to be able to get out of her bed. After the pins were removed ſhe could ſwallow ſolids, and recovered ſtrength ſufficient to go out again to ſervice in her former employment. She was hired as an under-ſervant in a gentleman's kitchen,

chen, but was ſoon obliged to quit her place, and apply for relief, any extraordinary motion aggravating her complaints, and occaſioning violent convulſions, from which ſhe did not recover for eight or nine hours. When ſhe came to the infirmary, ſhe appeared full of fleſh, of a ruddy complexion, and in perfect health, excepting the following complaints.

She had a pain in her right ſide, below the falſe ribs, which ſhe firſt felt immediately upon the removal of the pins from the *œſophagus*, and it continued to the time of her admiſſion at the hoſpital, but was moſt violent when ſhe moved the trunk of her body forwards round towards the left, or lifted up her right arm. At her admiſſion, and from the time of the removal of the pins, the hoarſeneſs ſhe was troubled with ſoon after the pins firſt ſtuck in her throat, continued; ſhe often ſpit up blood, and had a violent cough, by which, as well as by labour, or any exceſs of motion, the pain in her ſide being greatly aggravated, ſhe was obliged to ſit or fall down immediately, and could not recover herſelf, ſo as to be able to ſtand, in leſs than an hour. In theſe paroxyſms ſhe had always a pain in her head, was ſick at ſtomach, and frequently brought up blood.

Whilſt ſhe was in the infirmary, the violence of the pain three times occaſioned convulſion-fits, by which the *muſculus rectus ſuperior* of the right eye was ſo violently affected, that, notwithſtanding the eye was open, yet the pupil was entirely covered by the eye-lid; and, after one of theſe fits, continued ſo for a fortnight. The left eye was alſo inverted in the

ſame manner, but the conſtriction was removed in a week. When theſe ſpaſmodic affections left her, ſhe did not recover her eye-ſight for ſome days, the optic nerve being probably oppreſſed; but the left eye always recovered ſooner than the right, being never ſo ſtrongly convulſed. None of the other muſcles appeared to be affected, except in the paroxyſms.

While the pins were in the *œſophagus*, the ſurgeon was utterly at a loſs where to direct his inſtruments, as there was no certain indication where the pins were lodged. And the phyſician's practice could be only palliative, uſing bleeding, with anodyne and lubricating medicines, according as the various ſymptoms occaſionally required. In this manner things went on to the beginning of Auguſt, when a ſmall painful tumour, the ſize of a man's thumb, appeared upon the right ſhoulder, which diſappeared in the compaſs of a week without coming to ſuppuration. Afterwards ſuch another ſmall tumour appeared upon the left ſhoulder, which increaſed, and, by the care of Mr. Crump, the attending ſurgeon, was brought to ſuppuration, and opened by him, Auguſt 20, when a large table ſpoonful of matter was diſcharged. Upon removing the dreſſings, the next day, a larger quantity of matter flowed out, and with it iſſued one of the pins. Mr. Crump then examined with his probe if he could find either of the others, but could not: however, the day following, the other two pins were alſo diſcharged at the ſame wound. Theſe pins were all of the ſame length, each meaſuring five quarters of an inch. The wound at which theſe pins were diſcharged was upon the ſuperior part of the *ſcapula*. After the girl had received her cure, and

was

was diſcharged from the infirmary (which happened September the fourth), I compared her ſhoulder with Cowper's Anatomical Tables on the Muſcles; and, as near as I can gueſs, the wound was upon the fleſhy belly of the *trapeſius*. And yet the pain in the patient's ſide attended her as long as the pins remained in the wound, but left her ſoon after they were diſcharged, as did alſo her cough, and ſpitting of blood. Being obliged to lead a ſedentary life in the infirmary, and to keep herſelf as quiet as poſſible, her *catamenia* left her; but her ſpitting of blood could not be attributed to that defect, becauſe ſhe was very regular before her admiſſion, and yet ſhe had ſpit blood from the time the pins were removed from the *œſophagus*, which was ſome months before ſhe came to the infirmary.

It would be matter of conſiderable ſatisfaction, could the exact courſe be aſcertained which was taken by theſe pins, in their paſſage from the *œſophagus* to their exit at the left ſhoulder. From the cough and ſpitting of blood one ſhould ſuppoſe that the lungs were injured by them. From the pain under the falſe ribs, it may be imagined that the diaphragm was affected. And yet from their being diſcharged at the ſhoulder it may be preſumed, that neither of theſe parts were ever wounded; but that the pins, being forced through the ſubſtance of the *œſophagus* into the muſcles of the neck and ſhoulder, paſſed thence to the part whence they were diſcharged.

The firſt ſymptom obſervable upon the removal of theſe pins from the paſſage of the *œſophagus* was, that the patient immediately felt a pain in her right ſide,

ſide, below the falſe ribs, which was moſt violent when ſhe turned the trunk of her body forwards round towards the left, or lifted up her right arm. Now if the pins, being forced out of the *œſophagus*, penetrated the *ſerrati*, *rhomboides*, and *trapeſius* muſcles on the right ſide, this ſymptom muſt neceſſarily happen. For the *ſerrati* being muſcles of reſpiration, and the *ſerratus ſuperor poſticus* attached to the ſecond, third, fourth, fifth, and ſixth ribs; and the *ſerratus inferior poſticus* being attached to the tenth, eleventh, and the extremity of the twelfth ribs, a pain in the ſide will be produced by the conſtant efforts of reſpiration. And the office of theſe muſcles being to elevate the ribs, and draw down the arm, the pain in the ſide will be moſt ſenſibly felt whenever the right arm is lifted up; becauſe then the extremities of theſe muſcles, attached to the ribs, will be moſt tenſe. For although a wound may be given to a muſcle in its moſt fleſhy part, yet the irritation occaſioned by it will exert itſelf moſt forcibly in that part where there is the greateſt tenſion.

The *rhomboides* muſcle lying upon the *ſerratus ſuperior*, and the *trapeſius* being incumbent upon it, and all cloſely connected by the cellular membrane, they muſt all be in ſome degree affected by reſpiration. But the office of the *rhomboides* and *trapeſius* muſcles being to draw the arm downwards, and backwards, the pain in the ſide would be increaſed whenever the right arm and trunk of the body were turned forwards towards the left ſide.

Being thus, as we may ſuppoſe, arrived at the true cauſe of the pain in the ſide, the cough comes next under conſideration. And this will be found to pro-

ceed from the ſame cauſe that the cough of a pleuritic perſon does, only with this difference, that in one the *pleura* and intercoſtal muſcles are affected by an internal inflammation, by which reſpiration is diſturbed; in the other, the malady ariſes from irritation cauſed by an extraneous body. The effects are the ſame in both; reſpiration being impeded, nature endeavours to relieve herſelf by a cough, which increaſes the irritation and inflammation of the parts obſtructed; theſe again increaſe the violence of the cough: and thus, each being aggravated by the other, the lungs are often ſo violently agitated, that a blood-veſſel burſts, and thence blood is thrown up from the lungs, as was the caſe in the preſent inſtance.

Whoever conſiders the communication between the third pair of nerves, the intercoſtal, the cardiac, and the recurrents, together with the other nerves dependent upon them, will eaſily perceive the cauſe of the violent ſpaſm upon the eyes, the ſickneſs at ſtomach, and the general convulſion, as being all primarily dependant upon the irritation given to the intercoſtal nerve on the right ſide. And it may be obſerved, that, although both the *motores oculorum* were affected, yet the right eye was convulſed moſt violently.

From the ſymptoms attending this uncommon caſe, it is reaſonable to conclude, that the three pins were all of them at the ſame time forced from the *œſophagus* into the *ſerrati* muſcles on the right ſide, which immediately communicated an irritation, or impulſe, to the intercoſtal nerve, from whence aroſe the pain in the ſide, and thence the ſickneſs at ſtomach, and convulſions of the eyes and other parts.

But

But whatever caused the pain in the right ſide, upon the removal of the pins from the *œſophagus*, that cauſe continued to act until all the three pins were diſcharged at the left ſhoulder, for ſo long did the pain in the right ſide continue.

The thickneſs of the two *ſerrati*, the *rhomboides*, and *trapeſius* muſcles may be thought too great for pins five quarters of an inch long to penetrate all of them at the ſame time. But if it be obſerved, that one of the pins was diſcharged at a time when neither of the two others could be felt with the probe, it may be ſuppoſed, that one of the three paſſed into the *rhomboides*, and *trapeſius*, whilſt the two others remained in the *ſerrati*, and there continued until the firſt was diſcharged at the *trapeſius*; after which they took the ſame courſe, and were diſcharged at the ſame outlet.

Thus might we give a very probable account of this extraordinary caſe, had the pins been diſcharged at the right ſhoulder, but they were diſcharged at the left. By thoſe who think that, the nerves communicating with one another, the cauſe and effect produced may be on oppoſite ſides of the body, it may be ſaid, that the pins might be forced from the *œſophagus* into the muſcles of the left ſide, notwithſtanding the pain was felt in the right. This will not be generally allowed. Neither can I perceive any reaſon why a tumor exactly reſembling that from whence the pins were afterwards diſcharged at the left ſhoulder, ſhould ariſe upon the right, and diſperſe without coming to ſuppuration.

Since I drew out the above account, I have ſeen a caſe nearly ſimilar to it, recorded in the Philoſophical Tranſac-

Tranſactions, N° 461. A ſmall needle being lodged in a woman's left arm, about ſix inches below the ſhoulder, paſſed thence to her right breaſt, whence it was extracted many months after it firſt entered the body. About a month after the accident, ſhe felt a pain above the place where the needle run in, which extended up her ſhoulder. It laſted there three or four days, and then returned by fits. About 17 weeks before the needle was extracted, ſhe felt a pain at her ſtomach, was ſick, and had reachings to vomit. Theſe ſymptoms continued to afflict her (eſpecially in the morning), until within two days of the needle being extracted, at which time ſhe thought a pin had got into her right breaſt. This directed the ſurgeon to make an opening there, and he extracted the ſame needle that had entered at her arm from the part where the pricking pain was; after which ſhe had never any return of pain in her breaſt, ſtomach, ſhoulder, or arm.

If, upon peruſal of this caſe, you think it merits the attention of the curious, as corroborating the other, your recommendation of it to the Royal Society will be eſteemed an honour to,

SIR,

Your much obliged,

humble ſervant

Glouceſter, Sept. 1, 1768.

Dan. Lyſons.

IV. *A Letter from the Honourable* William Hamilton, *his Majesty's Envoy Extraordinary at* Naples, *to* Mathew Maty, *M. D. Sec. R. S. containing some farther Particulars on Mount* Vesuvius, *and other Volcanos in the Neighbourhood.*

Villa Angelica, near Mount Vesuvius,
October 4, 1768.

SIR,

Read February 2, 1769.

I HAVE but very lately received your last obliging letter, of the 5th of July, with the volume of Philosophical Transactions.

I must beg of you to express my satisfaction at the notice the Royal Society have been pleased to take of my accounts of the two last eruptions of Mount Vesuvius. Since I have been at my villa here, I have enquired of the inhabitants of the mountain after what they had seen during the last eruption. In my letter to Lord Morton, I mentioned nothing but what came immediately under my own observation; but as all the peasants here agree in their account of the terrible thunder and lightning, which lasted almost the whole time of the eruption, upon the mountain only; I think it a circumstance worth attending to. Besides the lightning, which perfectly resembled the common forked lightning, there were many meteors, like what are vulgarly called falling stars. A peasant,

ſant, in my neighbourhood, loſt eight hogs by the aſhes falling into the trough with their food: they grew giddy, and died in a few hours. The laſt day of the eruption, the aſhes, which fell abundantly upon the mountain, were as white almoſt as ſnow; and the old people here aſſure me, that is a ſure ſymptom of the eruption being at an end. Theſe circumſtances, being well atteſted, I thought worth relating.

It would require many years cloſe application, to give a proper and truly philoſophical account of the volcanos in the neighbourhood of Naples; but I am ſure ſuch a hiſtory might be given, ſupported by demonſtration, as would deſtroy every ſyſtem hitherto given upon this ſubject. We have here an opportunity of ſeeing volcanos in all their ſtates. I have been this ſummer in the iſland of Iſchia; it is about eighteen miles round, and its whole baſis is lava. The great mountain in it, near as high as Veſuvius, formerly called Epomeus, and now San Nicolo, I am convinced was thrown up by degrees; and I have no doubt in my own mind, but that the iſland itſelf roſe out of the ſea in the ſame manner as ſome of the Azores. I am of the ſame opinion with reſpect to Mount Veſuvius, and all the high grounds near Naples; as having not yet ſeen, in any one place, what can be called virgin earth. I had the pleaſure of ſeeing a well ſunk, a few days ago, near my villa, which is, as you know, at the foot of Veſuvius, and cloſe by the ſea-ſide. At 25 feet below the level of the ſea they came to a ſtratum of lava, and God knows how much deeper they might have ſtill found other lavas. The ſoil all round the mountain, which is ſo fertile,

confifts of ftratas of lavas, afhes, pumice, and now-and-then a thin ftratum of good earth, which good earth is produced by the furface mouldering, and the rotting of the roots of plants, vines, &c. This is plainly to be feen at Pompeii, where they are now digging into the ruins of that ancient city; the houfes are covered, about 10 or 15 feet, with pumice and fragments of lava, fome of which weigh three pounds (which laft circumftance I mention to fhew, that, in a great eruption, Vefuvius has thrown ftones of this weight fix miles, which is its diftance from Pompeii, in a direct line); upon this ftratum of pumice, or *rapilli*, as they call them here, is a ftratum of excellent mould, about two feet thick, on which grow large trees, and excellent grapes. We have then the Solfaterra, which was certainly a volcano, and has ceafed emptying, for want of metallic particles, and over-abounding with fulphur. You may trace its lavas into the fea. We have the Lago d'Averno and the Lago d'Agnano, both of which were formerly volcanos; and Attroni, which ftill retains its form more than any of thefe. Its crater is walled round, and his Sicilian Majefty takes the diverfion of boar-hunting in this volcano; and neither his Majefty, or any one of his Court, ever dreamed of its former ftate. We have then that curious mountain, called Montagno Nuovo, near Puzzole, which rofe, in one night, out of the Lucrine Lake; it is about 150 feet high and three miles round. I do not think it more extraordinary, that Mount Vefuvius, in many ages, fhould rife above 2000 feet; when this mountain, as is well attefted, rofe in one night, no longer ago than the year 1538. I have a project, next fpring, of paffing fome days at Puzzole, and of

diffecting

diffecting this mountain, taking its meafures, and making drawings of its ftratas; for, I perceive, it is compofed of ftratas, like Mount Vefuvius, but without lavas. As this mountain is fo undoubtedly formed entirely from a plain, I fhould think my project may give light into the formation of many other mountains, that are at prefent thought to have been original, and are certainly not fo, if their ftrata correfpond with thofe of the Montagno Nuovo. I fhould be glad to know whether you think this project of mine will be ufeful; and, if you do, the refult of my obfervations may be the fubject of another letter.

I cannot have a greater pleafure than to employ my leifure hours in what may be of fome little ufe to mankind; and my lot has carried me into a country, which affords an ample field for obfervation. Upon the whole, if I was to eftablifh a fyftem, it would be, that *mountains are produced by volcanos, and not volcanos by mountains.*

I fear I have tired you: but the fubject of volcanos is fo favourite a one with me, that it has led me on I know not how: I fhall only add, that Vefuvius is quiet at prefent, tho' very hot at top, where there is a depofition of boiling fulphur. The lava that run in the Foffa Grande during the laft eruption, and is at leaft 200 feet thick, is not yet cool; a ftick, put into its crevices, takes fire immediately. On the fides of the crevices are fine cryftalline falts: as they are the pure falts, which exhale from the lava that has no communication with the interiour of the mountain, they may perhaps indicate the compofition of the lava. I have done. Let me only thank you for

the kind offers and expreſſions in your letter, and for the care you have had in ſetting off my preſent to the Muſeum to the beſt advantage; of which I have been told from many quarters.

I am,

SIR,

Your moſt obedient,

humble ſervant,

W. Hamilton.

Received January 9, 1769.

V. *A Letter to Dr.* William Watson, *F. R. S. from the Hon.* Daines Barrington, *F. R. S. on the Trees which are supposed to be indigenous in* Great Britain.

January 2, 1769.

DEAR SIR,

Read Feb. 9 and 16, 1769.

SINCE you sent me the specimen of supposed chesnut, which was taken from the old hall of Clifford's Inn, I have been at some pains to examine into the authorities for the prevailing notion, with regard to this being an indigenous tree; as also with relation to some others, which are generally conceived to be of the native growth of Great Britain.

But, before I enter into other particulars, I shall venture to lay down some general rules, from which it may be decided, whether a tree is indigenous or not, in any country.

1. They must grow in large masses, and cover considerable tracts of ground; nor must such woods end abruptly, by a sudden change to other trees, except the situation and strata become totally different.

2. If the tree grows kindly in copses, and shoots from the stool, it must for ever continue in such a wood,

wood, unleſs grubbed up with the greateſt care; nor is it then eaſily extirpated.

3. The ſeed of ſuch tree muſt ripen kindly: nature never plants but where a ſucceſſion may be eaſily continued, and in the greateſt profuſion.

Laſtly; Many places in every country muſt receive their appellation from indigenous trees which grow there; as no circumſtance is more ſtriking, when a tract of ground is to be deſcribed or diſtinguiſhed: hence ſo many towns, villages, and farms are named from the oak and aſh, which are the moſt common trees of Great Britain.

When the inſtances of this are ſingular, it will prove directly the contrary; as I hope to ſhew ſoon with regard to the cheſnut and the box.

Having ventured to premiſe theſe general rules, by which it may be determined, whether a tree hath been planted by the hand of nature or not; I ſhall now begin, by conſidering the proofs which are commonly relied upon, with regard to the Spaniſh, or ſweet, cheſnuts being indigenous in Great Britain.

And, firſt, the very name of Spaniſh ſeems moſt ſtrongly to indicate the country from which it was originally introduced here, as much as if a particular ſpecies of oak was known in Spain by the name of the Engliſh oak.

There may be ſome doubts, perhaps, whether this tree is really a native of any part of Europe, as Pliny informs us, cheſnuts were brought from Sardis to Italy, and that they were improved in taſte by Tiberius, who took particular delight in cultivating them *.

* Pliny, lib. XV. cap. xxiii.

I have

I have alſo been informed by you, that in Spain the cheſnut trees, deſtined to produce the beſt fruit, are engrafted upon the wild cheſnut, and that the French call the common ſort *chataignier*, and the improved one *maronier*.

Though ſo much hath been ſaid of late, with regard to the excellence of this wood for building, I cannot, upon inquiry, find that it is greatly prized for this purpoſe either in Spain, Italy, or the South of France; but is chiefly valued for the fruit, which forms a conſiderable article of food for the inhabitants, as well as of exportation.

I likewiſe cannot hear that this tree is to be found in any conſiderable maſſes, till the traveller is at leaſt two hundred miles to the ſouth of Paris.

With us the nuts by no means ripen kindly, though I have ſometimes eaten them very good from Engliſh trees.

In Scotland, neither the walnut nor cheſnut produce good fruit, though there are ſome very fine and promiſing timber trees, of the latter kind, at the Earl of Bredalbane's, in the Highlands.

All theſe circumſtances ſeem to afford a ſtrong inference, that the Spaniſh cheſnut cannot be a native of Great Britain; but I muſt now conſider the proofs which are generally adduced to the contrary.

Mr. Miller (in his Gardener's Dictionary) hath endeavoured to prove, that the Spaniſh cheſnut grew in great profuſion to the northward of London, by a citation from Fitz-Stevens, which only implies, that there were large foreſts in the neighbourhood of the metropolis, without either the cheſnut, or any other tree, being ſpecified. "Proxime patet foreſta ingens,

" saltus numerosi, ferarum latebræ, cervorum, dama-
" rum, aprorum, et taurorum sylvestrium."

Mr. Miller also mentions, that some stumps of decayed chesnuts have been seen not far from the metropolis; he does not, however, particularize the spot, and it should seem therefore, that he had received this information from others.

Most antiquaries suppose, that Old London was chiefly built with this kind of timber from these forests; there is not the least appearance, however, of any such tree at present within twenty miles of London, which may not be accounted for, as being of infinitely a more modern introduction than the time of Henry the Second, when Fitz-Stevens wrote.

I remember the having once been present myself, when a wager on this head was won; it being supposed that a small specimen of a beam, from a very ancient house in Chancery-lane, was of this wood; which turned out to be nothing but common oak.

When you, therefore, lately put into my hands another such specimen of supposed chesnut, from the old hall of Clifford's Inn, I knew it immediately to be only the common oak.

As I had, however, at that time, an opportunity of proving this to a demonstration, by sending into the country for part of an oaken beam from a very ancient stable, and also a piece of Spanish chesnut, which grew near; I shall, for your further satisfaction, send three specimens, which you will compare where the wood hath been cut transversly, and where they are marked with ink at top and bottom.

The large irregular piece, marked *Cl.*, is the supposed chesnut from Clifford's Inn.

The

The ſpecimen, marked *O*, is from an oaken beam of an ancient ſtable.

The ſpecimen, marked *C*, is from a Spaniſh cheſnut.

I think it muſt immediately appear to any one, on inſpection, that the ſpecimens *Cl.* and *O* agree in the grain and texture of the wood, and that ſpecimen *C* is evidently of a different kind.

Upon weighing alſo the ſpecimens *C* and *O*, which are exactly of the ſame ſize, the oak turned out to be heavier than the cheſnut, by one fourth.

With regard to this latter difference between the two woods, it may be proper to inform you, that the ſpecimen of cheſnut was taken from a young tree; the grain of the oak muſt have therefore been cloſer than that of the cheſnut; but, on the other hand, it muſt be recollected, that there muſt have been a very conſiderable evaporation from the oaken beam during a long courſe of years.

Dr. Ducarel, in his Anglo-Norman Antiquities*, hath inſerted a note of ſome length, to prove, that Old London was not only built with cheſnut timber, but that there ſtill continues a large tract of cheſnut woods near Sittingbourn in Kent, which he conceives to be a full demonſtration, that this tree is indigenous in England.

I had no ſooner read this account, than I determined to examine theſe woods myſelf, as well as what trees might be found in their neighbourhood.

The reſult of a very minute inſpection of them is, that I found thoſe parts which conſiſt of Spaniſh

* P. 96.

 cheſnut

chesnut to be planted in beds or rows, about five yards distant from each other; nor are there any scattering trees to introduce them, which is what must be expected near woods of natural growth.

I shall now proceed to answer Dr. Ducarel's other arguments, with regard to the Spanish chesnut's being an indigenous tree in this country.

He first mentions a grant *Decimæ Castanearum in Denâ*, which he supposes to mean the forest of Dean.

Upon looking into Spelman's Index Villaris, I find no less than two-and-twenty towns and villages which bear the name of *Dean*. Why, therefore, it should mean the Forest, rather than any of these places, is not so obvious; especially when, considering the vast tract of ground included within this forest, the grant must have been of so very extensive a nature.

Supposing it to be the tithe *Castanearum* in any particular parish, it will amount to no more than a grant of the tithe of walnuts would do, which we know to be a tree originally of foreign growth.

The fruit of a small number either of walnuts or chesnuts is very valuable, if near a considerable town. I have been informed, that a grove, not exceeding an acre, of the latter, at Beachworth Castle in Surrey, hath sometimes produced upwards of ten pounds, at the London market, when the season happened to be favourable, and the nuts ripened kindly.

I should suppose that this grove of chesnuts, from their size, may be about two hundred years growth; and they already begin to decay very much at the tops, being what the woodwards term stag-headed.

If

If it be ſtill contended, however, that this grant of tithe includes the whole foreſt of Dean, I have been in almoſt every part of it, and can take upon myſelf to ſay, that there are not the leaſt veſtiges of any ſuch tree at preſent.

Dr. Ducarel next relies upon a manor in the neighbourhood of Sittingbourne being called Chaſtenye or Caſtenye, from the circumſtance of its being ſuppoſed to be amongſt cheſnut woods.

This, however, is a ſingle inſtance of ſuch a name to any place in England; and therefore the cheſnuts being indigenous can be no more inferred from it, than that box naturally grows in this country, from the name of Box-hill, in Surrey.

Now we happen to know that this hill was ſo called from an Earl of Arundel's having introduced this tree there, in the time of James or Charles the Firſt*; and, from many circumſtances, I ſhould ſuppoſe that the cheſnut plantations near Sittingbourne are not of a much more ancient date.

Dr. Ducarel then mentions two very fine cheſnut-trees, which grow at Hagley in Worceſterſhire; this, however, only proves, that the owner of that eſtate, ſome time ago, might think it worth while to plant them either for their beauty or their fruit.

* "This place (viz. Box-hill) was firſt planted by that famous antiquary (the Earl of Arundel), with box wood, deſigning to have built a houſe there; but want of water made him alter his reſolution, and build one at Albury, hard by; now belonging to the Earl of Aylesford." Journey through England, vol. I. printed in 1722.

See alſo the Article Box-Hill, in an Account of the Environs of London, printed for Dodſley.

The oldeſt tree we have any account of, perhaps in Europe, is a Spaniſh cheſnut which grows in a court at Tortworth in Glouceſterſhire: it is ſuppoſed by Evelyn and Bradley to have been planted in the time of King John, from mention of it in deeds of that antiquity.

I have, however, procured more accurate information from Lord *Ducie*, to whom this tree belongs; and find that the notion of its great age reſts merely upon a very uncertain tradition.

But although we ſhould ſuppoſe it to have been planted in the time of King John, it affords no ſtronger argument of the tree's being indigenous, than thoſe mentioned by Dr. Ducarel to grow at Hagley; eſpecially as there are no ſtraggling cheſnuts to be found in the neighbourhood of either of theſe places.

In further proof that the cheſnut formerly grew in this country, we are told, that the roof of Weſtminſter-hall, Boſton church in Lincolnſhire, and many others, conſiſt of this wood; not becauſe any one hath found it to be ſo upon examination, but becauſe there are no cobwebs upon ſuch roofs.

Sometimes alſo, to account for ſpiders not harbouring in them, it is ſuppoſed that the timber is not Engliſh, but Iriſh oak; in ſhort, recourſe is had to any extraordinary and uncommon material, to ſolve the ſingularity of there being no cobwebs on theſe roofs.

Having examined ſeveral ancient cielings with regard to this circumſtance, I take the cauſe of the ſpiders not reſorting to many of them to be the following:

This

This insect is known to subsist chiefly on the small flies which he surprizes in his nets; the consequence is, that he will no more be at the trouble of spreading his web where flies cannot be expected, than a fowler will lay his nets in a place where there is no resort of birds.

It is believed that few of the fly tribe are found at any great height from the ground, as they may be supposed to prey upon still smaller microscopical insects, which would be the sport of the winds at any considerable elevation. They are not therefore formed by nature for a high flight.

If one may be allowed also to argue from what is observed with regard to the smallest birds; neither the European wren nor the American humming-bird are ever seen upon any thing higher than a shrub.

Besides this, no fly is scarcely ever to be found but where there is a good deal of light and sun-shine; consequently a wide wooden roof (be it of what material it may) is the most improper place that the spider can lay his snares in.

If such roof therefore is dark, though it is at the same time very low, no flies will haunt it; for a proof of which I may refer you to the cloisters at Lincoln, or any gloomy cellar, though it may be above ground, and have windows which give it a certain degree of light.

Hence also spiders webs are more common on whitewash than on wainscot, especially if it be painted of a dark colour.

Having dwelt thus long upon the point of the Spanish chesnut's being indigenous or not, I shall

now trouble you with ſome obſervations relative to the Pine commonly called the Scotch Fir, which certainly is not to be found in any part of England at preſent, except where the plantation appears moſt evidently to be of modern date.

Cæſar, indeed, informs us, that no ſort of fir was to be ſeen in this country at the time of his invaſion.

It is well known that he made no very far advanced marches; and his obſervations are, perhaps, more to be depended upon with regard to military operations, than what might rather engage the attention of a botaniſt.

There are, however, ſo many well-atteſted facts, both by Camden and others *, of firs being found at a very conſiderable depth under the ſurface of the ground, that one cannot with-hold one's aſſent to them, extraordinary as it may appear at preſent, when throughout England we have no ſuch trees, which afford the leaſt grounds to contend that they are of indigenous growth.

If theſe indiſputable facts could want the addition of my poor teſtimony, I happened to ſee near Loch Rannoch, and in other parts of the Highlands of Scotland, ſubterraneous firs, which had been lately dug out.

I procured a labourer to chip off ſome parts of theſe trees, which ſmelt moſt ſtrongly reſinous; there could be no doubt therefore, from this circumſtance, but that they were firs, as well as from the grain of

* See Camden in Lancaſhire, and Phil. Tranſ. N° 67, where ſuch ſubterraneous firs are ſaid to be found in great quantities in the iſland of Axholm in Lincolnſhire.

the wood: the poor people in thoſe parts uſe ſmall pieces of them for candles.

There were, however, no fir woods near any of thoſe places, in the Highlands, where I happened to ſee theſe ſubterraneous trees; and, indeed, the indigenous ones are by no means ſo common as is generally apprehended.

Though what I have laſt mentioned may, perhaps, make many imagine, that the timber found under ground muſt have been ſome other tree, which ſtill continues to grow in the neighbourhood; yet I think there may be two cauſes aſſigned, why theſe bog-firs may be found in places where there is no ſuch tree at preſent.

The firſt is, that no pine or fir ever ſhoots from the ſtool; and the ſecond, that, being a reſinous wood, it is very eaſily ſet on fire by lightning, after a dry ſummer; and thus whole tracts of them may be deſtroyed without their revegetating.

I was, indeed, informed by an old man at Ranoch-Bridge, that his grandfather uſed to mention a tradition of the fir wood in that neighbourhood having continued burning for a conſiderable time, and that the Iriſh came over to ſee the conflagration.

A wood of this kind is ſtill growing near the weſtern end of Loch Ranoch, but it is ſeven or eight miles from the place where I ſaw the ſubterraneous trees, near which there was ſcarcely any other wood but birch.

There ſeems to be little doubt, therefore, that the fir was formerly an indigenous tree in the northern parts of England; nor does this contradict any of the

 rules

rules which I have ventured to lay down, as they have been found in great maſſes under ground, and their not continuing to grow in the ſame ſpot or neighbourhood hath been endeavoured to be accounted for.

As I travel a good deal during the ſummer, and attend to matters of this ſort, I ſhall now venture to mention ſome other trees, which do not ſeem to be indigenous, though they are commonly conceived to be ſo, as well as by ſome great botaniſts, who have treated of Engliſh plants and trees.

I cannot think that the elm, which we ſee every where, is indigenous. My reaſons are, that I have never ſeen it out of a hedge-row, avenue, or clump, though it is a tree which ſhoots vigorouſly from the ſtool: I have likewiſe never obſerved any ſeeds on this tree, though you have lately informed me, that they ſtand on very ſhort footſtalks, and that the bloſſom in the ſpring is of a pale red.

Upon looking into Mr. Miller's Dictionary, I find he likewiſe aſſerts that this tree bears ſeed; but at the ſame time mentions its being difficult, if not impracticable, to ſupply the nurſeries from it; which ſhews that it ſcarcely ever comes to maturity in this climate.

The Wych (or broad-leaved) elm, however, is certainly of natural growth in this country; though it is more common in Scotland, and the northern parts of England, than in the ſouthern counties.

For the ſame reaſons, I cannot allow the lime to be indigenous, though in ſome years the ſeed becomes mature.

The

The greater part of the limes, which we now ſee in every part of England, have been planted ſince the time of Charles the Second, and were introduced by a French gardener, whoſe name was Le Notre, at the ſame time with the horſe-cheſnut.

There are, however, at More-Park in Hertfordſhire, ſix or ſeven of theſe trees, which appear to be the growth of ſome centuries; ſo ſmall a number, as well as two or three limes of great antiquity on the banks of the river Neath in Glamorganſhire, only prove, that they were planted by ſome gentleman, either for variety, or perhaps for the fragrancy of the flower. I ſhould indeed rather ſuppoſe, that we owe moſt of the foreign trees which may have been introduced into England ſome centuries ago, to the alien abbots and priors, who, on ſpecial occaſions, ſometimes viſited their benefices in this country.

The greater Maple (or Sycamore, as it is improperly called) is certainly a foreign tree, though very common in Scotland, the northern counties of England, and ſome parts of Wales.

I never ſaw the tree but in a hedge-row, avenue, or clump; it muſt be admitted, however, that its ſeed comes to its full perfection in almoſt every year.

I have already mentioned a reaſon for the box not being conſidered as indigenous; to which I muſt now add, that no ſuch place as Box-hill is to be found in Saxton's maps, which were completed in the reign of Queen Elizabeth; nor is it taken notice of by Gerard, in his Article Box, who botanized ſo much in the neighbourhood of London.

We ſhould, indeed, find great profuſion of this ſhrub in Berkſhire, as Aſſer Menevenſis hath derived the name of that county from the word Berroc: I could never either ſee, or hear of, box in any part of it, except where it had been uſed in a parterre, to ſeparate the beds of flowers.

I have likewiſe looked into Benſon's Saxon Vocabulary, nor can I find any ſuch word as Berroc. Mr. Lye's new Saxon Dictionary furniſhes alſo no ſuch Article; on the contrary, he derives Baꞃocꞅciꞃe a quadam nudâ quercu in foreſta de Vinðeꞅouꞃe ad quam ſolebant provinciales convenire. He cites for this Joh. Bromton 801.

I ſhall now mention ſome trees and ſhrubs, which I have doubts whether they are natives of Great-Britain or not, though they are ſo conſidered by the Botaniſts; I cannot pretend, however, to be ſo poſitive as in ſome of the former articles.

I never ſaw the Yew where it grew in large maſſes, or appeared to have been ſown by the hand of nature. The moſt conſiderable numbers which I have happened to meet with are on ſome of the Surrey hills: theſe, however, ſcarcely in any inſtance, cover more than an acre of ground.

If I ſhould be right in ſuppoſing that this tree is of foreign growth, it may then be, perhaps, contended, that we have no ever-green tree or ſhrub which is indigenous, except the Holly, the Juniper, and the Ivy.

Every church-yard, indeed, proves that this tree hath been for many centuries introduced into England; it ſeems, however, very extraordinary that we ſhould have no account when, or for what purpoſe,

this

this ſo very general a practice hath ſo long prevailed with us.

As far as I can be informed, after very diligent inquiries, it is peculiar to England; and I never ſaw but one yew-tree in a Scotch church-yard; which was of ſo extraordinary a ſize, that you will forgive me, I am ſure, for mentioning it, though it hath no relation to the main purpoſe of my letter.

It continues to vegetate at preſent in the church-yard of Glen-Lyon, a valley that takes its name from a river which runs through it, and empties itſelf into the Tay not far eaſtward from Taymouth, a moſt capital and picturesque ſeat of the Earl of Bredalbane's.

I meaſured the circumference of this yew twice, and therefore cannot be miſtaken, when I inform you that it amounted to fifty-two feet. Nothing ſcarcely now remains but the outward bark, which hath been ſeparated by the centre of the tree's decaying within theſe twenty years. What ſtill appears, however, is thirty-four feet in circumference.

This, therefore, is, perhaps, the largeſt tree we have any account of; as the great cheſnut at Tortworth, in Glouceſterſhire, was only 51 feet in circumference, when meaſured very accurately forty years ago by Greening, the father of the preſent gardener of that name.

To the catalogue of doubtful trees, I muſt alſo add the Abele, or White Poplar, having never ſeen it growing according to the rules which I have

ventured to lay down with regard to indigenous trees *.

I have likewiſe my ſuſpicions with regard to the Privet and Spindle tree; but theſe I muſt ſubmit to your more accurate obſervations; and am,

Dear Sir,

Your moſt faithful

humble ſervant,

Daines Barrington.

* I believe it hath hitherto eſcaped the botaniſts, that it is only in the laſt ſhoots of this tree, that the gloſſy and ſtriking white is to be ſeen on the reverſe of the leaf.

VI. *An*

VI. *An Account of a Caſe in which the upper Head of the* Os Humeri *was ſawed off, a large Portion of the bone afterwards exfoliated, and yet the entire Motion of the Limb was preſerved. By Mr.* White, *Surgeon, at* Mancheſter. *Communicated by Mr.* Watſon, *F.R.S.*

Read February 9, 1769.

EDMUND POLLIT, of Sterling, near Cockey Moor, in this county, aged 16, of a ſcrophulous habit of body, was admitted into the Mancheſter infirmary, April 6, 1768. The account I received with him was, that he had been ſuddenly ſeized, about a fortnight before, with a violent inflammation in his left ſhoulder, which threatened a mortification, but at laſt terminated in a large abſceſs, which was opened with a lancet a few days before his admiſſion. The orifice was ſituated near the *axilla*, upon the lower edge of the *pectoralis major*; and through it, I could diſtinctly feel the head of the *os humeri* totally diveſted of its burſal ligament. The matter, which was very offenſive, and in great quantity, had made its way down to the middle of the *humerus*, and had likewiſe burſt out at another orifice juſt below the *proceſſus acromion*; through which the head of the *os humeri* might eaſily be ſeen. The whole arm and hand were ſwelled

ſwelled to twice their natural ſize, and were entirely uſeleſs to him; he ſuffered much pain, and the abſorption of the matter had brought on hectic ſymptoms, ſuch as, night-ſweats, *diarrhœa*, quick pulſe, and loſs of appetite, which had extremely emaciated him.

Under theſe very dangerous circumſtances, there ſeemed no reſource but from an operation The common one in theſe caſes, that of taking off the arm at the articulation with the *ſcapula*, appeared dreadful both in the firſt inſtance and in its conſequences. I therefore propoſed the following operation, from which I expected many advantages; and performed it on the 14th of the ſame month.

I began my inciſion at that orifice which was ſituated juſt below the *proceſſus acromion*, and carried it down to the middle of the *humerus*, by which all the ſubjacent bone was brought into view. I then took hold of the patient's elbow, and eaſily forced the head of the *humerus* out of its ſocket, and brought it ſo entirely out of the wound, that I readily graſped the whole head in my left hand, and held it there till I had ſawn it off (ſee Tab. I. Fig. 1.), with a common amputation-ſaw, having firſt applied a paſteboard card betwixt the bones and the ſkin. I had taken the precaution of placing an aſſiſtant on whom I could depend, with a compreſs juſt above the clavicle, to ſtop the circulation in the artery, if I ſhould have the misfortune to cut or lacerate it; but no accident of any kind happened, and the patient did not loſe more than two or three ounces of blood, only a ſmall artery which partly ſurrounds the joint being wounded, which was eaſily ſecured.

He

He was remarkably eaſy after the operation, and reſted well that night; the diſcharge diminiſhed every day, the ſwelling gradually abated, his appetite returned, and all his hectic ſymptoms vaniſhed. In about five or ſix weeks, I perceived the parts, from which the bone had been taken, had acquired a conſiderable degree of firmneſs, and he was able to lift a pretty large weight in his hand. At the end of two months, I found that a large piece of the whole ſubſtance of the bone, that had been denuded by the matter, and afterwards expoſed to the air, was now ready to ſeparate from the ſound, and with a pair of forceps I eaſily removed it (Fig. II.). After this exfoliation, the wound healed very faſt; and on Auguſt 15th he was diſcharged perfectly cured. On comparing this arm with the other, it was not quite an inch ſhorter; he has the perfect uſe of it, and can not only elevate his arm to any height, but can likewiſe perform the rotatory motion as well as ever. The figure of the arm is no ways altered; and from the uſe he has of it, from its appearance to the eye, and to the touch, I think I may ſafely ſay, the head, neck, and part of the body of the *os humeri* are actually regenerated.

I did not make uſe of any ſplints, machine, or bandage, during the cure, to confine the limb ſtrictly in one certain ſituation, nor was his arm ever dreſſed in bed, but ſitting in a chair, and, as ſoon as he could bear it, ſtanding up, with his body leaning forwards, to give room for applying the bandages, which were no more than juſt neceſſary to retain the dreſſings; and to this method I attribute the preſervation of the motion of the joint, which could not have

been ſo well effected any other way; but the joint, in all probability, would have remained ſtiff, and formed an *anchyloſis*, if it had not been allowed to play about.

REMARKS.

Though from this operation I hoped for many advantages preferable to the amputation of the limb at the *ſcapula*, yet my moſt ſanguine expectations fell greatly ſhort of the ſucceſs attending it. I did not flatter myſelf with the hopes of a moveable joint, or that the length of the limb would be ſo nearly preſerved, when there was a loſs of above four inches of the whole ſubſtance of the bone without any other bone to ſupport it as in the leg and fore-arm; and where the dreadful condition of the arm at the time of the operation prevented me from making uſe of any machine to keep it extended *. But I ſuppoſe the weight of the arm was in this caſe in ſome meaſure ſufficient to counterbalance the contractile power of the muſcles, for his arm was only ſuſpended by a common ſling, and the patient not at all confined to his bed.

I could not help being ſurprized to find ſo much ſtrength and firmneſs in the parts as evidently ſhewed a regeneration of the bone before the lower part had

* After the extraction of three inches and ten lines of the *os humeri*, M. Le Cat made uſe of a machine to keep the upper and lower pieces of the bone at their proper diſtances. He has given a deſcription of the caſe, and a figure of the machine, in vol. LVI. of the Philoſ. Tranſact. p. 270.

exfoliated,

exfoliated, or even before it had begun to loosen. This osseous matter could not proceed from the *scapula*, the glenoid cavity of that bone not being divested of its cartilage; could it then possibly escape from the end of the sound bone, before the morbid part had begun to separate from it? or are there any vessels that could convey the bony matter, and deposit it in the place of what had been removed *? These are points that I will not pretend to decide absolutely, but I am much inclined to the latter opinion †.

* Mr. Gooch, in his volume of Cases and Practical Remarks, relating the case of a compound fracture of the leg, where a very considerable portion of the *tibia* was sawed off, says, "In about "three weeks I was sensible, as were also several surgeons whom "curiosity led to see so uncommon a case, that the substance "which grew in the space of five inches entirely void of bone, "had acquired in the middle only a greater degree of solidity "than flesh; which circumstance not agreeing with the general "received notion of the generation of *callus*, we proved beyond "dispute, with a sharp pointed instrument, and we observed that "the ossification was gradually formed from that central point, "which was considerably advanced before any exfoliation was "cast off the ends of the divided bone. In less than four "months, the whole space was so well supplied with the *callus*, "or rather new bone, that he was able to raise his leg when the "bandage was off, without its bending."

Cases and Remarks, *new Edit.* p. 287.

† In universum in sanguine materies est apta producendo ossi, quæ adeo frequenter in cellulosum spatium intimum, interque convexam superficiem membranæ intimæ arteriarum, concavamque membranæ musculosæ extremitatem effunditur, et caseosa primo, inde callosa, quasi coriacea, demum osseæ squamæ fit simillima.

Halleri Elem. Physiolog. tom. VIII. p. 316.

——— Calli in ossibus non fracturas solas, sed amissa integra ossa farcientes, fiunt ex liquido glutinoso, pulsu proximarum arteriarum compacto, &c. Halleri Primæ Lineæ, p. 148.

See further Haller's Pathological Observations, Obs. xlvii.

Is it not probable that there is a regeneration of the cartilage as well as the bone? for it is well known to every-body converſant in anatomy, that the ends of ſome bones, which are joined to no others, are covered with cartilages; but theſe are never wanting on the ends, and in the cavities of ſuch bones as are deſigned for motion; and I cannot ſee in this caſe how the motion could be preſerved ſo complete without a cartilage; and indeed without a burſal ligament, or ſomething analogous to it, to contain the *ſynovia*, and keep the bone in its place.

As this is the firſt operation of the kind that has been performed, or at leaſt made public, I thought the relation of it might poſſibly conduce to the improvement of the art. That ingenious ſurgeon, Mr. Gooch, has indeed related three inſtances of the heads of bones being ſawed off in compound luxations. In one of theſe caſes the lower heads of the *tibia* and *fibula* were ſawn off, in another that of the *radius*, and in the third that of the ſecond bone of the thumb; but theſe were, in many reſpects, different from the preſent caſe.

I believe it will ſeldom happen that this operation will not be greatly preferable to the amputation of the arm at the *ſcapula*, as this laſt is generally performed for a *caries* of the upper head of the *os humeri*; and as the preſervation of a limb is always of the utmoſt conſequence, and what every ſurgeon of the leaſt humanity would at all times wiſh for, but particularly where (as in this caſe) the whole limb and its actions are preſerved entire, the cure no ways protracted, and the danger of the operation moſt undoubtedly leſs. For though amputation is often indiſpenſably neceſ-

ſary,

Fig. 1.

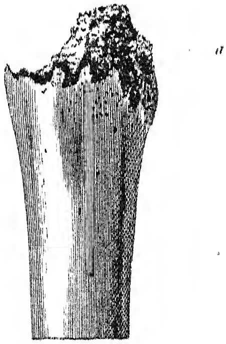


Fig. 2.

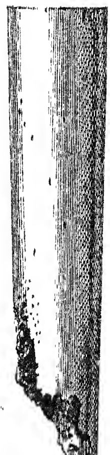
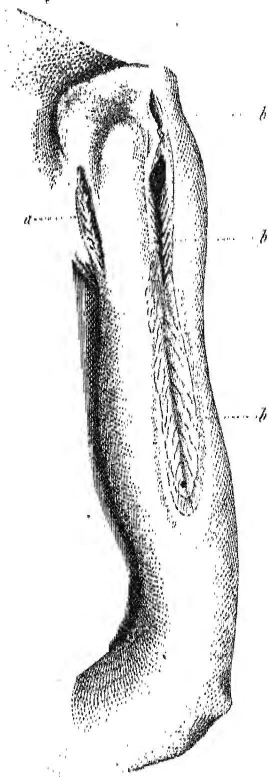


Fig. 3.



J. Mynde sculp.

ſary, and frequently attended with little danger or inconvenience when only part of a limb is removed; yet where the whole is loſt, the danger is greatly increaſed, and the loſs irreparable.

I have frequently performed this operation on dead ſubjects, and where the parts had not been diſeaſed, and never found any difficulty; and from the diſſection of the parts had no reaſon to doubt of ſucceſs in a living ſubject, where the ligaments and muſcles are more ſupple, and the matter, by inſinuating itſelf betwixt the bone and integuments, has made leſs diſſection neceſſary.

I have likewiſe, in a dead ſubject, made an inciſion on the external ſide of the hip joint, and continued the inciſion down below the great *trochanter*, then cutting through the burſal ligament, and bringing the knee inwards, the upper head of the *os femoris* was forced out of its ſocket, and eaſily ſawed off; and I have no doubt but this operation might be performed upon a living ſubject with great proſpect of ſucceſs.

The Royal Academy of Surgery at Paris propoſed for a prize queſtion, Whether amputation of the thigh at its articulation with the *os innominatum* was ever adviſeable: but was I under a neceſſity of performing this operation, or that which I have been deſcribing, I ſhould not heſitate a moment which to prefer.

C. White.

TAB. I. FIG. I.

That part of the bone which was ſawed off.

a. The head of the bone corroded by the matter.

FIG.

FIG. II.

The piece of bone which exfoliated. Both pieces together were five inches in length, four of which were of the whole ſubſtance.

FIG. III.

The arm as it appeared after the wound was healed.

a. The cicatrix of the firſt inciſion.

b.b.b. The cicatrix of the inciſion made to bring out the head of the bone.

N. B. The drawings from which this plate was engraved were made by Mr. Aikin, my pupil, who attended at the operation, and during the whole cure.

Received February 9, 1769.

VII. *Letters from the Rev. Dr.* William Borlaſe, *F. R. S. Rector of* Ludgvan *in* Cornwall, *to* Charles Morton, *M. D. F. R. S. and from Mr.* Roſewarne, *of* Truro, *to Dr.* Borlaſe; *giving an Account of a Specimen of Native Tin found in* Cornwall, *and now depoſited in the* Muſeum *of the* Royal Society.

Ludgvan, Jan. 30, 1769.

SIR,

Read February 16, 1769.

PERHAPS you may not have forgot that, in the year 1765, I ſent a ſpecimen of native tin, to be depoſited in the Royal Society's Muſeum; and though the account of it, publiſhed in the Tranſactions of the year following, was not ſuch as I wiſh, yet I am ſteadily intent on paying my duty to the Society, and obviating (as far as lies in my power) all doubts relating to natural knowledge.

Laſt poſt I received a letter from Mr. Roſewarne, of this county, with an account of his having met with another ſpecimen of native tin; and I ſend it you incloſed, for your inſpection, to be returned at your

your leifure. This young gentleman is a confider-able merchant, efpecially verfed in tin affairs, with a great fhare of quicknefs and underftanding in the fu-fion of metals; and I refer it to you, whether it may not be proper to introduce him at the next meeting of the Royal Society, by your means, and requeft him there to fhew that rare fpecimen which he carries with him to town.

He, indeed, intends (you'll find by his letter) to bring it back with him; but if this be only to let me have the infpection of it, and, through my hands, to depofit it in your Mufeum, I fhall eafily difpenfe with every ceremony of that kind; and I write him by this or next poft, that (as it is fmall, and may mif-carry, or be miflaid in fuch long journies) I intreat him to let it reft, for the fatisfaction of the doubtful, in your Mufeum; and, as I hope to prevail, let it re-main with the fample I fent, but in Mr. Rofewarne's name.

As every acquifition in natural hiftory will, I know, be very acceptable to you and the gentlemen of the Society, for whom I have the greateft refpect, I make the lefs apology for giving you this trouble, and remain,

SIR,

Your moft obedient fervant,

William Borlafe.

Received

Received February 9, 1769.

To the Rev. Doctor William Borlase.

Rev. Sir, Truro, Jan. 27, 1769.

Read February 16, 1769.

I HAVE the pleasure to acquaint you, that I have at last met with a specimen of native tin, which is so evidently so at first sight as not to admit of the least doubt or objection. The description of it is this: some streamers in the parish of Luxilion brought in a parcel of tin ore to a blowing-house I am concerned in at Sthurtle; amongst the parcel was a great number of tin diamonds of a most beautiful nature, of the rozin kind; one was eminently superior to the rest, being almost transparent, and seemed to have something in the center which, through the stone, looked like gold. This induced me to break the stone; which was no sooner done than I found it to be native tin, in the very center of the diamond. The specimen is so small, that I am at a loss which way to send it for your inspection, for fear it should be lost. I shall set out for London on Monday next, and intend carrying this curiosity with me. I'll not leave it behind me; but when I come back you shall see it, and through you deposit it with the Royal Society, for the satisfaction of the curious. I am,

Rev. Sir,

Your obliged and faithful

humble servant,

Henry Rosewarne.

VIII. *An Account of an Essay on the Origin of a natural Paper, found near the City of* Cortona *in* Tuscany. *In a Letter from* John Strange, *Esq; F. R. S. to* Mathew Maty, *M. D. Sec. R. S.*

Dear Sir,

Read February 16, 1769.

MY letter to Mr. Coltellini, secretary to the Botanical Academy of Cortona, concerning the origin of a natural paper found in the neighbourhood of that city (which, with some specimens of paper, you obligingly presented to the Royal Society in my name), being written in a foreign language, and but little known; I have thought proper to give you the following short account of it, together with some additional remarks, which I have made since its publication.

In August 1763, some low grounds, in a farm about four miles south-west of Cortona, which had been flooded, were found covered with a substance very much like a finer sort of common brown paper. A circumstantial account of the fact was communicated to the public the September following, in a letter from the said Mr. Coltellini to Dr. Lami, professor of theology at Florence.

This account surprised, and excited the curiosity of the naturalists in Italy; and various were the conjectures

jectures offered upon the occaſion. The prevailing opinion, however, attributed the formation of this paper to a caſual aggregate of the fibres of different kinds of filamentous plants, collected together by the waters, and left on the ſurface of the ground at their retreat. This ſuppoſition ſeemed plauſible enough, ſince ſuch a mechaniſm could be produced only by filamentous plants; moſt of which are commonly the ſpontaneous productions of ſuch low, marſhy ground. But upon conſidering that, in the paper manufactures of different countries, various degrees and methods of maceration are requiſite, according to the reſpective qualities of the fibres of different plants; it appeared to me very difficult to conceive, that a paper of ſo delicate and uniform a texture as that of Cortona ſhould owe its origin to ſo complicated and remote a cauſe.

To bring the matter in queſtion to a more certain iſſue, I therefore thought it neceſſary to examine the threads of this paper with a good microſcope; and, agreeable to the opinion I had entertained, found them to conſiſt of mere filaments of the common ſpecies of *Conferva*, without the intervention of any other plant whatſoever. It was eaſy enough to aſcertain the identity of the Conferva, the filaments of which it is compoſed being of a peculiar ſtructure, and very different from thoſe of any terreſtrial plant; beſides, as they are ſolitary in their natural ſtate, they undergo no other alteration by the above mechaniſm, than the loſs of the *parenchyma* that inveſts them, the ſtructure of the filaments themſelves remaining as perfect as ever.

To confeſs the truth, I was but very ſuperficially acquainted with this ſpecies of Conferva till I had made the above diſcovery; ſince the deſcriptions of it, which we find in the books of botany, by no means afford an adequate idea of the ſtructure of the plant. Dillenius (1), in his deſcription of it, pretending to correct Pliny, for a ſuppoſed impropriety in the term *fiſtuloſæ denſitatis*, ſays, that there is no cavity obſervable either in this or other larger ſpecies of Conferva, excepting, perhaps, in his *Conferva dichotoma* (2); in which he is certainly miſtaken; ſince the filaments of the common Conferva, when examined with a good microſcope, evidently appear to be capillary tubes divided at equal diſtances by parallel *ſepta* or diaphragms, exactly like the 25th ſpecies of the ſame *genus* in Dillenius's Tables. Pliny's (3) epithet, therefore, ſo far from being improper, is a real characteriſtic of the thing in queſtion.

As the ſyſtematical botaniſts generally take their leading characters from the external figures of plants, we need not be ſurpriſed to find them inaccurate in their deſcriptions of the ſmaller tribes; more eſpecially as they neglect the uſe of proper glaſſes, by which alone they can acquire a knowledge of them. Dillenius and Linnæus himſelf have both been led into miſtakes, from this omiſſion. The former, in the preface to his *Hiſtoria Muſcorum*, confeſſes, that he made uſe of common glaſſes only, in order that the figures of the ſmaller plants, which he was to

(1) Hiſt. Muſc. Gen. 1. Ord. 1. Sp. 1, 2.
(2) Ib. Gen. 2. Sp. 9. (3) Hiſt. Nat. lib. XXVII. cap. viii.

repreſent

represent in his Tables, might not deviate too much from the natural appearance of the plants themselves to the naked eye: and it is pretty evident that the glasses he used were but of moderate powers, since, besides other mistakes, they left him quite undetermined whether his 4th and 5th species of Conferva had ramifications or not, though this very distinction forms a separate series in the first *Ordo*. Linnæus's (4) generical character of this plant is certainly less defective than that given by Dillenius, inasmuch as he takes notice of the tubercula omitted by the former, and calls the fibres of the Conferva *capillary*; but as he does not expressly say, whether these fibres are tubes or not, and takes no notice of the *septa* or diaphragms distributed at equal distances along them, I apprehend that he equally neglected examining the plant with proper glasses. Perhaps he adopted the term *capillaris* from professor Van Royen's Synonyme, which he quotes; especially since, in his divisions and specific characters of the Conferva, he has fallen into the same mistakes with Dillenius, whom he chiefly followed in his class of the *Cryptogamia*.

If the systematical botanists have not therefore acquired an adequate knowledge of the structure of the minuter Conferva, by neglecting to use proper glasses, their descriptions of these plants must necessarily be imperfect.

The specimens of paper, which I sent you with the copy of my letter, are,

First, A specimen of the natural paper of Cortona.

Secondly, An artificial paper made of the same substance with the natural paper of Cortona; which

(4) Gen. Plant. Cl. 24.

ſubſtance I prove (5) to be the common Conferva; but as the plant, by miſtake, was not kept long enough in maceration, the parenchymous matter, which ought to have been ſeparated from it, is in part ſtill remaining, and gives the paper a greeniſh colour, beſides making it very brittle.

Thirdly, A ſpecimen of a much better and ſtronger paper made of the ſame Conferva, by Sir Alexander Dick, baronet, near Edinburgh; and I remember ſeeing others of the ſame ſort, but of inferior quality, made by Monſ. *Guettard*, of Paris; an account of which has been already printed (6).

Fourthly, A ſpecimen of another artificial paper, which I made of the *Geniſta Juncea* macerated in warm water, and prepared afterwards in the common manner. I do not recollect, that this ſubſtance was ever tried before; neither is the *Geniſta Juncea* inſerted in the liſt of filamentous plants publiſhed by Monſieur *De la Lande* (7). This anecdote, though Vitruvius (8) recommended it for ſimilar uſes ſo many centuries ago, was new to Monſieur *De la Lande*, when I had the opportunity of communicating it to him at Paris on my return from Italy in 1764.

Theſe matters are, I confeſs, of more ſpeculation and curioſity than uſe; but as they have given occaſion, in the courſe of my inquiry, to ſome phyſical and botanical remarks, I thought they might not prove wholly unacceptable to the learned members of the Royal Society; and ſhould think myſelf very happy, if other gentlemen, of greater knowledge

(5) See the Table of References to the Figures.
(6) Journ. Œcon. Avril, 1761.
(7) Art de faire du Papier.
(8) Archit. lib. VII. cap. iii.

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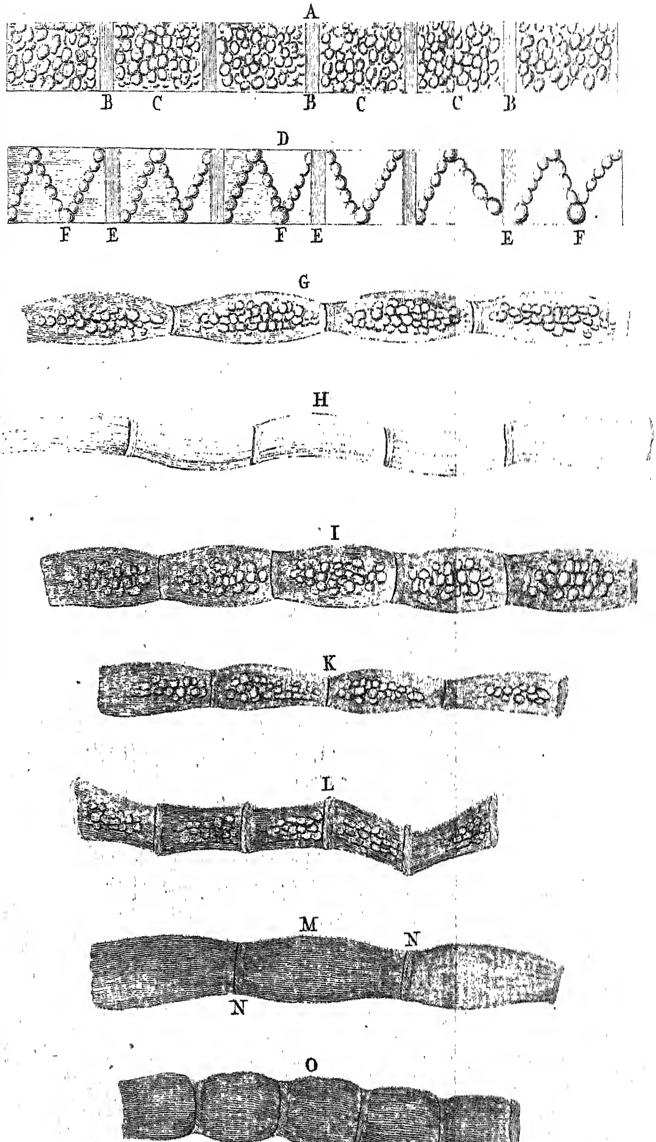
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and abilities than myſelf, ſhould be hereby encouraged to purſue the above hints, in order to obtain a more accurate knowledge of a genus of plants hitherto but imperfectly deſcribed, and perhaps leſs known than any other of the *Cryptogamia* claſs. I am,

Dear Sir,

Your moſt obedient,

and humble ſervant,

John Strange.

Appearances of the Conferva Plinii, viewed in different ſtates with the microſcope; with references to the plate. See Tab. II.

A. Conferva Plinii, in its natural vigorous ſtate, of a deep green colour; taken from the fountain in the middle of the botanical garden at Florence.

B. B. B. Tranſverſe membranes or diaphragms, apparently of a ſpongy nature, and tranſparent.

C. C. C. Parenchyma which gives the deep green colour to this plant, replete with globular appearances, like air-bladders.

D. Conferva Plinii, of a yellowiſh green colour; taken from a fountain in Marquis Grifoni's garden at Florence. This plant was in the firſt ſtage of putrefaction, a great part of its parenchyma being diſſolved and ſeparated from it: hence its degradation of colour.

E. E. E. The

E. E. E. The tranſparent diaphragms.

F. F. F. Appearance of the parenchyma with fewer, but apparently regular veſicles of air.

G. Conferva Plinii A. dried between two pieces of glaſs, and thereby contracted at the joints. The parenchyma of the plant had ſtuck theſe pieces of glaſs ſo faſt together, that they were with difficulty ſeparated.

H. Conferva Plinii D. dried between two pieces of glaſs. The apparently longitudinal fibres are nothing but foldings of the membranous tube; for, on moiſtening the plant, and moving the pieces of glaſs in different directions, theſe foldings increaſe or decreaſe arbitrarily.

I. K. L. Appearances of Conferva Plinii, taken from three different places in the canals about Piſa, afterwards ſteeped for fifteen days in common water, then extended upon paper to dry, and put a ſecond time into water for an hour.

M. Appearance of a thread of the natural paper of Cortona, with the diaphragms N N appearing opaque.

O. A thread of the ſame paper, ſteeped for an hour in common water.

Received February 9, 1769.

IX. *Experiments on the lateral Force of Electrical Explosions. By* Joseph Priestley, *LL. D. F. R. S.*

Read February 23, 1769.

BEING informed, in accounts of damages done by lightning, of persons and things being removed to considerable distances, without receiving any hurt, I was excited to try whether I could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power; but I do not know that this effect has ever been so much as taken notice of by any electrician. The experiments I presently found to be very easy; and I think it not difficult to ascertain the cause of this striking effect, and the manner in which it is produced.

If pieces of cork, wood, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar, or battery, they will not fail to be moved out of their places, at the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described in the printed account of my experiments, and large corks be strewed along, or near the path intended for it, it is surprizing to

to obſerve with what violence they will be driven about the room; and this diſperſion is in all directions from the center of the exploſion; and it makes no difference whether the rods, between which it is made, be ſharp-pointed or otherwiſe.

The effect of this lateral force is very remarkable in attempts to fire gunpowder in electrical exploſions. If the gunpowder be confined ever ſo cloſe in quills or cartridges, and they be held faſt in viſes, yet, when the exploſion is made in the center of them, it will ſometimes happen, even when a wire has been melted in the midſt of the powder, and the fragments have been ſeen red-hot for ſome time in different parts of the room, that the powder has not been fired, or only a few grains of it, the reſt being diſperſed with great violence, part of it flying againſt the faces of perſons who aſſiſted in making the experiments. This circumſtance, together with the charcoal being a conducter of electricity, makes it ſo extremely difficult to fire gunpowder by electrical exploſions; and it is evidently owing to this lateral force, that parts of the melted wire fly ſo many ways, and to ſo great a diſtance from the place of exploſion.

This lateral force is exerted not only in the neighbourhood of an exploſion, when it is made between pieces of metal in the open air, but alſo when it is tranſmitted through wires that are not thick enough to conduct it perfectly; and the ſmaller the wire, and the more complete the fuſion, the greater is the diſperſion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the exploſion (in which caſe it generally aſſumes a duſky red, which laſts but for a moment), there

there was a ſmall diſperſion from every part of the wire, but by no means ſo great as it would have been if it had been melted, or only heated to a greater degree.

By a conſiderable number of trials I found, that a greater force of exploſion would move light bodies at a greater diſtance; but the ſmaller the bodies were, the leſs was this difference; ſo that I ſuppoſed, that if they had no weight at all, they would, probably, be moved at the ſame diſtance by the exploſion from any quantity of coated ſurface, charged equally high; but there was a great difference in the weights removed by different forces at the ſame diſtance. Placing the ſame piece of cork at the ſame diſtance from the place of exploſion, I found that the diſcharge of one jar removed it $\frac{1}{4}$th of an inch, two jars $1\frac{1}{4}$th, three $1\frac{3}{4}$ths, and four about two inches; ſo that I do not wonder at very heavy bodies being moved from their places, and to conſiderable diſtances, by ſtrong flaſhes of lightning.

That the immediate cauſe of this diſperſion of bodies in the neighbourhood of electrical exploſions is not their being ſuddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it, is, I think, evident from the following experiments and obſervations. I never obſerved the leaſt ſenſible attraction of theſe light bodies to the braſs rods, through which the exploſion paſſed, or to the electric matter paſſing between them, previous to this repulſion, though I uſed ſeveral methods which could not have failed to ſhew it, if there had been any ſuch thing. Sometimes I ſuſpended them in fine ſilken ſtrings, and obſerved

that they had contracted no electricity after they had been agitated in the manner described above. Sometimes I dipped them in turpentine, and observed that no part of it was found sticking either to the brass rods themselves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the explosion of a battery made ever so near to a brass rod did not so much as disturb the equilibrium of the electric fluid in the body itself: for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found that the balls were not in the least moved at the time of explosion, which they would have been, if part of the electric fluid, natural to the body, had been driven, though but for a moment, towards the opposite end. I also observed, that the effect was the same, when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds interposed between the explosion and the bodies removed by it, as paper, tin-foil, and even glass; for when some grains of gunpowder were put into a thin phial, close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I therefore think it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a *vacuum* of air in its passage; and this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation commu-

communicated to the thin ſubſtances, and to the air, and the light bodies placed beyond them.

The only objection to this hypotheſis is, that this lateral force is not ſo much leſs *in vacuo* as might be expected, when the air is ſuppoſed to receive the concuſſion firſt, and to communicate it to other bodies; but it muſt be conſidered, that the moſt perfect *vacuum* we can make with a pump is not free from air. I have tried to make this experiment in a Torricellian *vacuum*, but could not ſucceed at that time. Beſides, as the electric matter, of which an exploſion conſiſts, muſt take a wider path *in vacuo*, if not equally fill the whole ſpace, it may affect a body in its paſſage, without the intervention of any air. In condenſed air, this lateral force was not, as far as I could perceive, much increaſed.

Willing to feel what kind of an impulſe it was that acted upon bodies, when they were driven away by this lateral force of electricity; I held my finger near the path of an exploſion of the battery, paſſing over the ſurface of a green leaf, when I felt a ſtroke, as of ſomething puſhing againſt my finger. Several corks, placed in the ſame ſituation, were driven to a conſiderable diſtance by the ſame exploſion.

Recollecting that this power, which I now call the lateral force of electrical exploſions, muſt be the ſame with that which gives the concuſſion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal ſubſtances, over the ſurface of which it paſſes; and being determined to make a more ſatisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the exploſion paſs over the

leaf;

leaf; but the leaf was burſt, and torn to pieces, and the exploſion, paſſing over my hand, gave it a violent jar, the effect of which remained, in a kind of tingling, for ſome time.

Laſtly, in order to judge the moſt perfectly of this force, I laid a chain communicating with the outſide of the battery upon my bare arm, above the wriſt, and bringing the diſcharging rod near the fleſh, within about two inches and a half of the chain, I made the exploſion paſs over that quantity of the ſurface of the ſkin. Had I taken a greater diſtance, I was aware that the exploſion would have entered the fleſh; which, I was ſenſible, would have given a painful convulſion to the muſcles through which it paſſed. In this caſe the ſenſible effect was very different from that, being the ſame external concuſſion as before; and I have ſometimes thought, that the ſenſation is not diſagreeable. However, the hairs upon the ſkin were ſinged, and curled up along the whole path of the exploſion, and for the ſpace of about half an inch on each ſide of it: alſo the *papillæ pyramidales* of the ſkin were raiſed, as when a perſon is ſhivering with cold. This was alſo the caſe in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the exploſion, and the place on which the chain had lain, had a redneſs which remained till the next day. Sometimes the fleſh has contracted a blackneſs by this experiment, which has remained for a few hours.

X. *Various*

X. *Various Experiments on the Force of Electrical Explosions. By* Joseph Prieftley, *LL. D. F. R. S.*

Read March 2, 1769.

MAKING the explofion of a battery pafs over the furface of a green cabbage-leaf, I obferved that it left a track near $\frac{1}{4}$th of an inch in breadth, exceedingly well defined, and diftinguifhable by a difference of colour from the reft of the leaf. Along this path, alfo, the firmnefs of texture in the leaf was entirely deftroyed, that part becoming quite flexible, like a piece of cloth. Prefently after, it turned yellow, grew withered, and became perfectly brittle.

Willing to try the effect of this explofion paffing along the furface of other fubftances, I laid a piece of common window-glafs on the path, preffed by a weight of fix ounces; but it was fhattered to pieces, and totally difperfed, together with the leaf on which it lay. Placing the black fide of a piece of cork-wood upon it, preffed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it about half an inch in breadth, and a quarter of an inch in depth. Laying the fmooth cut furface of the piece of cork, it was furrowed all the way, as if it had been cut with a file, but not near fo deep as before. Many of the fmall pieces, which had been rubbed off in the explofion,

plofion, remained in the furrow. Alfo the fubftance of the cork feemed to be fhattered, and it was eafily rubbed off, a little way into it.

I made this explofion on the furface of fome red wine, in a fmall difh, and kept a part of the fame quantity expofed in a fimilar manner; but I could perceive no difference between them after feveral days.

The track of an electrical explofion on the furface of the cabbage-leaf, being fo well defined, fuggefted an experiment to afcertain whether there was any fenfible momentum in the electric fluid, when it is rufhing with violence from one fide of a battery to the other. For this purpofe I made the explofion pafs over the leaves when they were cut in right and acute angles; fo that the fhorteft path, from the infide to the outfide of the battery, was to turn clofe at the angle; and obferved that it was not diverted from its courfe, in the leaft degree, by the rapidity of its own motion, but that it had turned exactly at the angle, and kept as clofe to the oppofite fide, as if the motion had begun at the angle. The electric matter had, however, been evidently attracted by the veins of the cabbage-leaf, having purfued them a little way, at leaft having fenfibly affected them, wherever it met with them in its paffage.

This experiment fuggefted another, intended to determine whether the force of an explofion was at all diminifhed by being diverted from a right-lined courfe, and made to turn in a great number of angles. To do this, I firft found, by a great number of trials, what length of a fmall iron wire I was able to melt with a battery of about twenty fquare feet, in the middle

middle of a circuit of about three yards of brafs wire, confiderably thicker than the iron, and ftretched in two right lines, fufpended on filken ftrings. The length of the iron wire, melted in thefe circumftances, was about three inches. I then took the fame brafs wire, and fixing pins into a board of baked wood, twifted it about them, making it turn in a very great number of acute angles, and I put three inches of the fame iron wire in the middle of this crooked circuit, that I had done in the ftraight one; fo that the electric matter in the explofion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the fame length of iron wire was melted in thefe circumftances as in the other, and not the leaft difference was perceived in the force.

But though the form of the wire through which an explofion paffed, made no difference in its force, I found a very remarkable difference occafioned by the length of the circuit in wires of the fame thicknefs; and which, I own, furprized me very much.

In order to afcertain the practicability of firing mines by electrical explofions, I took twenty two yards of fmall brafs wire (but fo thick, however, that I could not have melted the leaft part of it by the force of any battery I have ever conftructed), and extending it along a dry boarded floor, with a fmall piece of iron wire, and a cartridge of gunpowder about it, in the place that was moft remote from the battery, I found that, upon the difcharge, the wire was not melted, nor the gunpowder exploded; alfo the report was very faint. In other circumftances, a charge of the fame battery was able to melt more than

 nine

nine inches of this iron wire; and this ſame cartridge was eaſily fired near the battery, connected with ſhorter pieces of the ſame braſs wire; ſo that the diminution of force muſt have been owing to the length of the circuit.

In the place of this ſmall braſs wire, I ſubſtituted an iron wire one fifth of an inch thick, when about half an inch of the ſmall iron wire was exploded; ſo that the force was not leſſened ſo much in a circuit of the thick iron wire, as it had been in one of the ſmall braſs wires. In order to judge how much of the force might be loſt by nearer circuits, conſiſting of leſs perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immerſed. The effect was, that the ſmall iron wire was only made red-hot, but not exploded as before.

Being ſenſible how much depended upon avoiding leſſer circuits, whereby part of the fire of an exploſion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted, in the remaining experiments, I inſulated half the circuit of iron wire. There was no occaſion for inſulating the whole circuit; for if there was but one paſſage to, or from the middle of it, there could be but one from, or to it. In this method it was eaſy to aſcertain what loſs of force was occaſioned by the length of the circuit, as every other circumſtance was carefully excluded; and it preſently appeared to be very conſiderable; for tho' I could melt nine inches of the ſmall iron wire at the diſtance of fifteen yards from the battery; when I tried twenty yards, I found that I was juſt able to make

ſix

ſix inches of it red-hot. The battery in theſe experiments was in the houſe, and the wires of which the circuit conſiſted were conveyed by ſilken ſtrings into a garden adjoining to the houſe.

Mentioning this loſs of force occaſioned by the length of the circuit in electrical exploſions to Dr. Franklin, he told me that the ſame obſervations had occurred to him, and that he had alſo been diſappointed in an attempt to fire gunpowder at a diſtance from his battery.

Struck with this appearance, I endeavoured to aſcertain the quantity of this obſtruction, by trying what other courſes the electric fire would chuſe preferably to a long metallic circuit. In the firſt place, taking about a yard of the ſmall braſs wire, mentioned above, I diſpoſed it in the manner deſcribed below, connecting one of the ends with the outſide of the battery, and the other with the inſide. In the firſt place, I brought the parts *a* and *b* (near the two extremities) into contact, and, upon the diſcharge, found there had been a fuſion in that place, and that a great part of the fire had taken the ſhorter circuit, though it had been obliged to quit the wire in one place, and enter it again in another. Afterwards I removed the parts *a* and *b* to a ſmall diſtance from one another, and, upon the exploſion, obſerved a ſtrong ſpark paſs between them. Removing them to greater and greater diſtances, I found the exploſion to paſs above one third of an inch in the air, rather than make the circuit of the continued wire. Uſing a longer and ſmaller iron wire, the paſſage through the air exceeded half an inch. I then took four or five yards of iron wire one tenth of an inch thick, when

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the paſſage through the air was ſtill half an inch; and taking three yards and a half of wire that was one fifth of an inch thick, the ſpark in the air was half an inch, and ſometimes near three quarters of an inch. Making uſe of only half the length of this wire, the paſſage through the air was only half that diſtance, or $\frac{1}{4}$th of an inch. When I kept the place of near contact about the middle of this wire, and made the exploſion at the extremities of the whole wire, I was obliged to bring them about as near again, *i. e.* to little more than one eighth of an inch, before the paſſage would be through the air; ſo that the force of the whole exploſion muſt have been greatly weakened by its paſſing through ſo much of the wire. Laſtly, I took a pair of kitchen tongs, the legs of which were two feet, and the ſmalleſt part of them above half an inch in diameter; when the circuit was made about one ſixth of an inch in the air (for at that diſtance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

Notwithſtanding this evident paſſage of the electric matter through the air, at the ſame time that a metallic circuit was provided for it; it was certain that the whole of the charge did not paſs in the air: for when I extended $\frac{1}{3}$d of an inch of ſmall iron wire between *a* and *b*, it was only made red-hot by the diſcharge; whereas above two inches of it would have been exploded, if there had been no other metallic circuit at all.

As the electric fire meets with ſo much obſtruction in paſſing through a circuit of iron of this thickneſs, I make no doubt but that it is conſiderably obſtructed

in

in paſſing through metallic circuits of any thickneſs whatever; and that it would prefer a very ſhort paſſage thro' the air, if they were made even of no great length. In this method the different degrees of conducting power in different metals may be tried, uſing metallic circuits of the ſame length and thickneſs, and obſerving the difference of the paſſage through the air in each. N. B. A common jar anſwers as well, in theſe experiments, as a large battery. It is evident, from many experiments, that the whole fire of an exploſion does not paſs in the ſhorteſt and beſt circuit; but that, if inferior circuits be open, part will paſs in them at the ſame time. Of this I made the following ſatisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; ſo that the parts of it made two circuits for the diſcharge, which I could vary at pleaſure; and I obſerved that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being conſiderable flaſhes between the links of the remoteſt part of the chain. If the charge was weak, it paſſed in the ſhorteſt circuit only.

It is evident, that when the wires of a battery are not in cloſe contact, there muſt be ſome loſs of force in

in the diſcharge; but this never appeared to me to be very conſiderable. In order to aſcertain it by experiment, I firſt found, by repeated trials, what length of a piece of iron wire I was able to melt with a battery conſiſting of twenty jars, with the wires and connecting rods quite looſe, and a chain to join the rods belonging to each row of jars, which is the manner in which I have generally conſtructed them. In theſe circumſtances, I found the battery was able to melt ſomething more than two inches and a half of the wire. I then ſoddered the wires of each jar to the rod which connected them, and alſo ſoddered another rod to all theſe, inſtead of the chain which I had uſed; ſo that I avoided near a hundred ſparks in the diſcharge, at each of which there muſt have been ſome loſs of force; but I did not find, after many trials, that the ſtrength of the battery had been thereby ſenſibly diminiſhed: for I could not melt three inches of the ſame piece of wire in theſe circumſtances. It was only made red-hot, which is equivalent to the melting and exploding of little more than two inches and a half.

XI. *Abstract of a Letter from* Stephen De Vifme, *Efq; at* Canton, *in* China, *to* Henry Baker, *F. R. S. containing an Account of an Earthquake at* Macao, *and a ſhort Deſcription of a ſingular Species of Monkeys without Tails, found in the interior Part of* Bengal. *Communicated by Mr.* Baker.

Canton, January 7, 1768.

DEAR SIR,

Read March 9, 1769.

THE following account of an earthquake, at Macao, was ſent me by a friend at that place, in a letter, dated November 23, 1767. His words are: " Laſt night, at 50 " minutes after nine o'clock, we were all ſurprized " with a heavy ſhock of an earthquake, which con- " tinued above a minute. This ſhock was ſo great " that the houſe rocked, and I was afraid we were all " going down into the bowels of the earth. Another " ſhock we felt five minutes after eleven o'clock, but " not ſo great: and at three this morning another " pretty great. In all we have had five ſhocks, but " the firſt the greateſt. It came with a rolling, and " a dreadful noiſe in the air; ſo that at firſt ſome

" people

" people thought it to be the firing of guns, or thun-
" der at ſome diſtance. At the firſt ſhock I could
" hardly hold my feet; but, thank God, no bad ac-
" cident has happened; and I hope the Almighty
" will deliver us from any more of theſe frightful
" ſhocks. I was up almoſt the whole night. The
" wind was northerly, but faint, and it was ſultry
" hot; the ſky cloſe and cloudy, and not a ſtar to
" be ſeen. The oldeſt people here ſay, they never
" remember to have felt ſo violent a ſhock, and of
" ſo long continuance. The ſhips in the harbour
" ſhook and whirled about, and thoſe on board ima-
" gined at firſt that it had been a whirlwind."——
At Manilla earthquakes are often very violent, ſo as to overturn ſteeples, houſes, and other buildings; and I obſerved, when I was there, that, to prevent ſuch accidents, their timbers in building are placed in a very particular manner; they have no Attic ſtory, only ware-houſes, and one floor over them.

Perhaps the drawing, which I now ſend you, of a ſingular ſort of Monkies, male and female, may not prove unacceptable. Theſe animals are called Golok, or wild people, and are thought to be originally a mixture with the human kind, having no tails. They come out of the foreſts in the interior part of Bengal, from the country called Mevat. They inhabit the woods: their food is fruit, leaves, bark of trees, and milk: fleſh only when caught. They are very gentle, and extremely modeſt. They are of the height of a man; their teeth are as white as pearls; their legs and arms are in due proportion to their body, which is very genteel. Some of them

were

J. Mynde sc.

were brought to Decca; and what I now ſend you is a copy of the original drawing *. See TAB. III.

I remain, dear Sir,

Your affectionate

humble ſervant,

Stephen De Viſme.

* The monkey, of which Mr. De Viſme has ſent this drawing and ſhort account, ſeems to be very like, if not the ſame with the ape without a tail, deſcribed by Mr. De Buffon in the XIVth volume of the Hiſtoire Naturelle, p. 92, under the name of the Gibbon, which it bears in ſome parts of the Eaſt-Indies. This ſpecies is found, he ſays, along the coaſt of Coromandel, at Mallacca, on the Molucca iſlands, and upon the confines of China. It grows to be upwards of four feet, walks on its hind legs, and ſometimes on all four. The hair, with which it is covered, is either brown or black: round about its face is a circle of greyiſh hairs; its eyes are large, but ſunk in its head; its ears naked; its face flat, and of a copper colour. It is of a placid diſpoſition; its motions are gentle; it was fed with bread, fruits, almonds. But the moſt ſingular characteriſtic is, the great length of its arms; and though Mr. De Viſme takes no notice of this circumſtance in his deſcription, his drawing ſeems to indicate it; but in a leſs ſtriking manner than that of Mr. De Buffon, who adds, that, when the animal is upright, it can touch the grou d with its hands.

M. M.

XII. *A Letter from Mr.* John Robertson, *Lib. R. S. to* James West, *Esq; President of the Royal Society; containing the Demonstration of a Law of Motion, in the Case of a Body deflected by two Forces tending constantly to two fixed Points.*

SIR,

Read April 6, 1769.

THE late Mr. Machin (who was, for many years, secretary to the Royal Society, and Gresham professor of astronomy) gave to the editor of the English edition of Sir Isaac Newton's Principia, published in the year 1729, a tract entitled, "The Laws of the Moon's Motion "according to Gravity," which was annexed to that impression: Mr. Machin, in the Postscript to that tract, after apologizing for not mentioning the fundamental principles of the demonstration of the propositions relating to the Moon's motions, says, "Some "of which, I am apt to think, cannot easily be "proved to be either true or false, by any methods "which are now in common use."

One of these principles he gives in the following words:

"There is a law of motion which holds in the "case where a body is deflected by two forces, "tending constantly to two fixed points.

"Which

" Which is, *That the body, in ſuch a caſe, will*
" *deſcribe, by lines drawn from the two fixed points,*
" *equal ſolids in equal times, about the line joining the*
" *ſaid fixed points.*

And (after obſerving that Sir Iſaac Newton has proved, that Kepler's law of bodies deſcribing equal areas in equal times about the centers of their revolution, cannot hold, whenever the body has a gravity or force to any other than one and the ſame point) further ſays, " there ſeems to be wanting ſome ſuch
" law as I have here laid down, that may ſerve to
" explain the motions of the Moon and Satellites,
" which have a gravity towards two different centers."

About the year 1742, diſcourſing with that eminent mathematician, the late William Jones, Eſq; F. R. S. on the above-mentioned law, he ſhewed me its demonſtration, and permitted me to take a copy thereof; and as I conceive it to be highly worth preſerving, I now offer it to your conſideration, about giving it a place in the Philoſophical Tranſactions. I am,

SIR,

Your moſt humble ſervant,

February 27,
1769.

J. Robertſon.

TAB. IV.

FIG. I.

PROPOSITION.

IF a body (P), projected in a given direction, be constantly drawn towards two fixed points (S and T), which are not both in the same plane with the direction, the triangle (SPT), formed by right lines drawn from the body (P) to those fixed points (S and T), shall describe equal solids (STPP', STP'P''), in equal times, about the right line (ST) joining the said points.

FIG. 2. For, suppose a body projected in the direction PP', and acted upon by two centripetal forces towards the fixed points S and T; the angles P'PS, P'PT lying in different planes. Let the time be divided into equal moments.

In the first moment, suppose the body, by its given force, should move along the line PP'; and in the second moment, if no new force was added, it should continue to move in the same right line along $P'p = PP'$; but when the body has come to P', suppose it acted upon by the two centripetal forces, in the directions P'T, P'S; and let those forces be in proportion to that in the direction PP', as the lines $P't$, $P's$ to the line $P'p$ ($= P'P$).

With these three right lines $P'p$, $P't$, $P's$, complete the parallelopipid P'P''; and the body in P', being acted upon by these three forces, in the directions $P'p$,

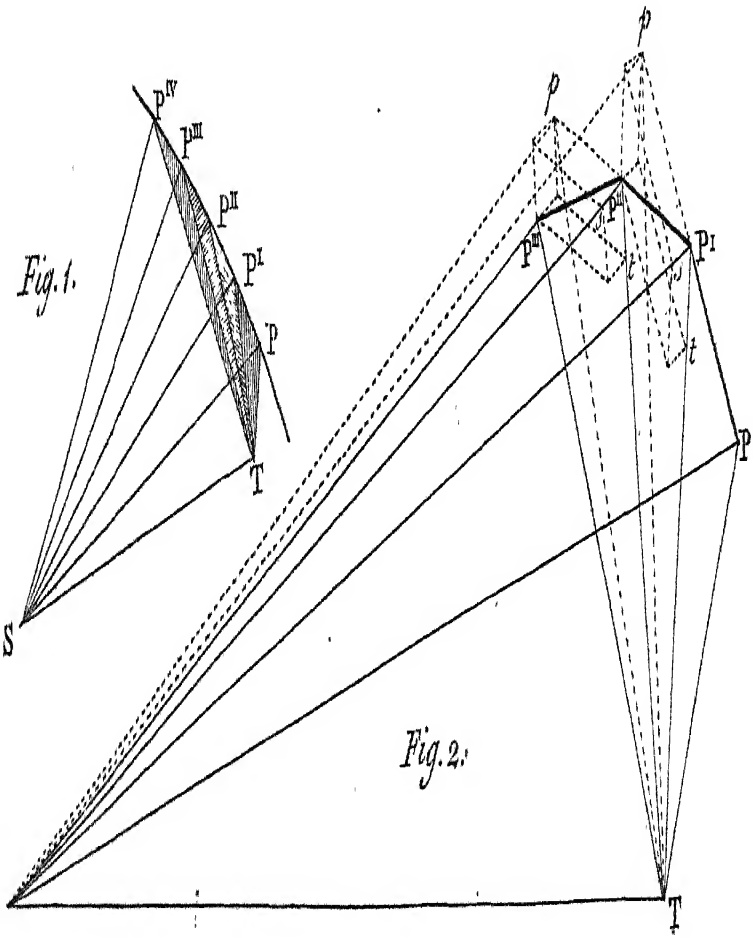


Fig. 1.

Fig. 2.

P'p, P't, P's, which forces being as these three lines, shall move along the diagonal of the parallelopipid made by these three lines; so that, in the second moment of time, the body, instead of moving from P' to p, shall move from P' to P''.

Draw the lines SP, SP'' and TP', TP'', as also Sp, Tp.

Now, the solid STPP' = solid STP'p; for they stand upon equal bases TP'P, TP'p, and have one common vertex S, or their common altitude is the perpendicular drawn from S to the plane PTp.

And the solid STP'P'' = solid STP'p; for they stand upon the same base STP', and lie between the same parallel planes pP'', st.

Therefore the solid STPP' = solid STP'P''.

In like manner, in the third moment of time, the body at P'' being acted upon by three forces, in the directions P'P'', P''S, P''T, shall move along the line P''P''', so as to make the solid STP''P''' = solid STP'P''; and so in all succeeding equal moments of time, the triangle formed by right lines drawn from the body to the two fixed points S, T, shall constantly describe little solids, each equal to the solid STPP'.

Therefore, the moments of the solids being proportional to the moments of the time in which they are described; the solid itself is proportional to the time in which it is described. Q. E. D.

Some difficulties may, perhaps, seem to arise upon a slight view of only particular cases of this proposition; but, it is conceived, all such must vanish, when the same is thoroughly considered.

For, as in two bodies T and S; if T is acted upon by S, so as to describe a right line, that is, if

if T falls directly upon S, no area can then be described by the right line connecting T and S; but yet, this is certainly one of the cases whereby S and T may possibly act upon each other.

So in three bodies, S, T, and P; if P moves in the same plane with S and T, no solid can then be described by the plane whose right lined sides are the lines connecting P to T and S; but yet, this must be one of the cases whereby S, T, and P, may possibly act upon each other.

Received April 13, 1769.

XIII. *A Letter from the Rev. Mr.* William Paxton, *Rector of* Buckland Brewer, *in the County of* Devon, *to Dr.* Milles, *Dean of* Exeter, *F. R. S. and Pr. S. A.*

Buckland Brewer, April 7, 1769.

REV. SIR,

Read April 20, 1769.

I HAD the favour of yours, and with great chearfulneſs comply with your requeſt, as far as faint words can expreſs what, in reality, is beyond the power of deſcription.

On Thurſday, the 2d of March, about four of the clock in the afternoon, a cloud, of a moſt uncommon blackneſs, gathered in the weſt-north-weſt, and, taking its courſe to eaſt-ſouth-eaſt, diffuſed a moſt prodigious darkneſs, accompanied with a very copious ſhower of hail. It paſſed immediately over the church tower (remarkable for the height both of its ſituation and ſtructure), and, burſting with incredible fury, poured forth an amazing body of fire, which threw down the ſouth-eaſt pinnacle on the church, and entering (as I ſuppoſe) at the breach, ſhivered a table on which the commandments were written, ſcorched and diſcoloured two tomb-ſtones, broke

broke the windows, and ſhattered the walls and roof to a great degree. The ſouth-eaſt corner ſuffered moſt; where it chiefly forced its way, and tore up the ground on the outſide, where it found vent. There is ſomething very extraordinary in the diſperſion of the ſtones of the pinnacle to every point of the compaſs, and to different diſtances; ſome of which were 700 pounds weight. I picked up one that weighed almoſt 8 pounds, at the diſtance of 60 perches from the church; and doubt not but others, and perhaps larger ſtones, were carried further: it may be worthy remark alſo, that ſeveral of the ſtones, ſome of which were not ſmall, though they appeared cloſe and firm, yet, on a very ſlight impreſſion of the fingers, mouldered into powder. The exploſion, on the opening of the cloud, was as inſtantaneous as terrible, and equalled the diſcharge of at leaſt a hundred cannon at once.

It is matter of great wonder, that not only the church, but that every houſe in the village, which trembled to its foundation, was not reduced to atoms, or lighted up into a general blaze; and yet, ſtupendous mercy! not a man, woman, child, or beaſt, received the leaſt hurt. I am,

SIR,

Your very obedient,

humble ſervant,

William Paxton.

Received Feb. 23, 1769.——Read April 20, 1769.

XIV. *Abstract from a Meteorological Register kept at the Royal Hospital near* Plymouth, *during the Year* 1768. *By* W. Farr, *M. D.* *Communicated by* W. Watson, *M. D. F. R. S.*

N. B. Observations were made twice every day at 9 a. m. and 11 p. m.——The state of the Thermometer, mentioned in the column, is taken from one placed within doors, in an open stair-case, free from the rays of the sun.

1768	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
January	3 Highest state of Barom. 1 Lowest state of D° 29 Highest state of Therm. 3 Lowest state of D°	30 .10 29 00 50 29	5·755	January 1st, after a rainy forenoon, wind S. E. the frost set in towards evening, wind N. and continued till the 8th, the lowest state of the thermometer without doors was 20, on the 3d and 4th. During the rest of the month much rain and squally weather, wind for the most part westerly. On the 20th, a violent storm, wind N. N. W. 4. when the Fame man of war was driven from her moorings in Hamoaze.

The

68	Barom. and Thermom.			Rain. Inches.	Weather, with Remarks.
ary 5	H. ſtate of Barom.	30	41		The firſt week clear and froſty weather, wind N. and moderate; loweſt ſtate of thermometer without doors 31; during the remainder of the month conſtant and heavy rains; from the 7th to the 28th but one fair day; wind, for the moſt part, W. and S. W. from the 9th to the 13th blew freſh.
10	L. ſtate of D°	29	20		
11	H. ſtate of Thermom.	53			
3	L. ſtate of D°	39		5.040	
h 19	H. ſtate of Barom.	30	40		This month proved remarkably dry; although a few drops of rain fell on the 1ſt, 12th, 15th, and 31ſt, yet the quantity was too inconſiderable to be collected in the receiver. From the 14th to the 18th the wind was W. the reſt of the month E. and N. E. with fine moderate froſty weather.
15	L. ſtate of D°	29	74		
18	H. ſtate of Thermom.	52			
3, 6, 7	L. ſtate of D°	39			
l 15	H. ſtate of Barom.	30	16		Wind continued eaſterly till the 10th, with dry weather; from that time, to the end of the month, for the moſt part, weſterly, with frequent ſqualls of wind and rain. 19th W. by N. 3. 28th S. 3.
29	L. ſtate of D°	29	27		
16	H. ſtate of Thermom.	56			
9	L. ſtate of D°	46		3.633	
					Air, the beginning of this month, extremely ſharp and cold, with wind freſh at N. E.; remarkable Aurora Borealis in the nights of the 3d, 4th, and

1768.		Barom. and Thermom.			Rain. Inches.	Weather, with Remarks.
May	19	H. ftate of Barom.	30	15		and 5th; on the 6th the Thermometer, which had ftood at 47 and 49 the preceding days, rofe, at once, to 60, and the air was very foft and warm; the Aurora Borealis appeared very vivid again on the 24th, with a large and bright halo round the Moon, when the Thermometer rofe to 70. Fair the whole month, except the 17th and 29th; wind variable.
	29	L. ftate of D°	29	17		
	24	H. ftate of Thermom.	70			
	1	L. ftate of D°	47		0.815	
June	2	H. ftate of Barom.	30	13		From the 6th of this month the rainy feafon commenced; wind, for the moft part, S. and S. W. 24th, a heavy thunder-ftorm, wind S. E. 28th, ftormy weather, with much rain, wind W. 3. Thermometer ftood at 57, and below 60, the greateft part of the month.
	9	L. ftate of D°	29	24		
	24	H. ftate of Thermom.	64			
	often	L. ftate of D°	57		4.340	
July	21	H. ftate of Barom.	30	08		1ft, very hot and fultry, with thunder; 2d, 9th, 13th, 15th, 16th, 20th, 30th, fair; the reft rainy. 7th, 12th, and 25th, very tempeftuous; wind, during the reft of the month, moderate, and, for the moft part, S. W.
	7	L. ftate of D°	29	29		
		H. ftate of Thermom.	64			
		L. ftate of D°	61		4.742	

1768.

1768.	Barom. and Thermom.			Rain. Inches.	Weather, with Remarks.
Auguſt 8	H. ſtate of Barom.	30	08		From the 13th to the 21ſt, weather hot and ſultry; thermometer being, for the moſt part, at 67, mornings and evenings (middle of the day not obſerved); from the 16th to the end of the month frequent ſhowers almoſt every day. Wind variable, but, for the moſt part, moderate.
23	L. ſtate of D°	29	40		
frequently	H. ſtate of Thermom.	67			
29	L. ſtate of D°	59		3.005	
Septemb. 14	H. ſtate of Barom.	30	10		From the 1ſt to the 25th, only four fair days, viz. 3d, 9th, 14th, and 20th, the reſt heavy and, often, conſtant rains, with ſqually weather, particularly 15th, 16th, 17th, wind W. 3. 22d, E. 3. From 25th to October 3d, fair weather.
16	L. ſtate of D°	29	05		
1, 11, 29	H. ſtate of Thermom.	63			
19	L. ſtate of D°	54		6.060	
October 19	H. ſtate of Barom.	30	10		Although ſo great a quantity of rain fell this month, yet there were 13 fair days; but the rains the reſt of the month were very heavy, particularly, from the 22d to the 31ſt, near four inches of rain were collected in the receiver. 5th, 6th, 18th, and 30th, ſtormy.
4	L. ſtate of D°	29	04		
7	H. ſtate of Thermom.	62			
28	L. ſtate of D°	49		7.350	
					4th, at 4 p. m. during heavy ſqualls of rain and wind W. by S. 3. the ☿ fell in the barometer to 29.11; towards evening wind changing to W. by N. it became fair; on the 5th, at 4 p. m. the

1768	Barom. and Thermom.			Rain. Inches.	Weather, with Remarks.
November 5 22 28 23	H. ſtate of Barom. L. ſtate of D° H. ſtate of Thermom. L. ſtate of D°	30 28 55 44	34 20	6.490	the ☿ roſe to 30.13, or above an inch, in 24 hours; and on the 6th 30.34. On the 22d, at 4 p. m. the ☿ fell in the barometer to 28.20, wind N. W. 2″, ſqually weather from the 21ſt to the 26th, but much more ſtormy on the 21ſt than afterwards. This was the loweſt ſtate of the barometer in theſe parts, ſince December 12, 1763, when the ☿ fell to 27.96. Only ſix fair days during the month.
December 12 2 23 14	H. ſtate of Barom. L. ſtate of D° H. ſtate of Thermom. L. ſtate of D°	30 28 83 34	31 94	3.985	1ſt, thunder-ſtorm in the night, wind S. 3. On the 14th, froſt ſet in very ſharp, wind E. by N. but the wind changing to S. E. on the 15th, rain ſucceeded. Thermometer, without doors, not obſerved. Wind, from the 7th to the 17th, eaſterly; the reſt of the month rain, for the moſt part, with weſterly winds. Eleven fair days this month.
				51.215	Total quantity of rain, during the year 1768.

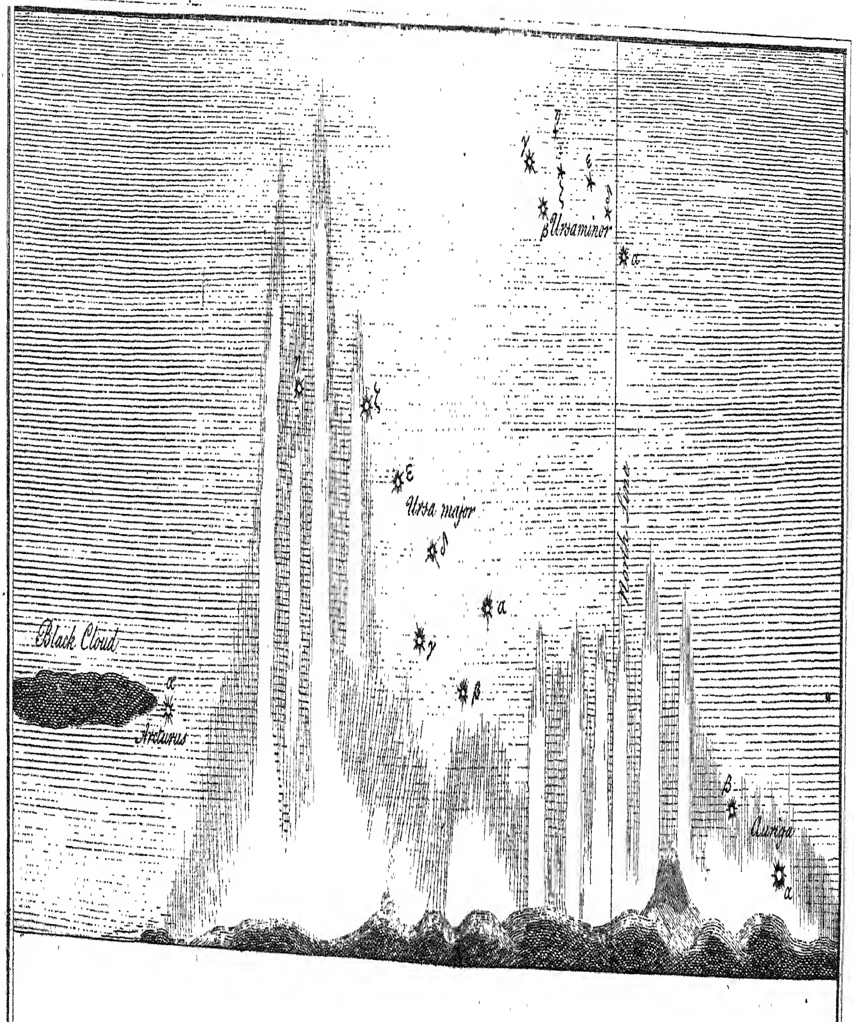
XV. *An*

XV. *An Account of a remarkable* Aurora Borealis, *observed at the Observatory of the Marine at* Paris, *by* M. Messier, *of the Royal Academy of Sciences, and F.R.S. Translated by* J. Bevis, *M. D. F.R.S.*

Read May 25, 1769.

THE morning of the 6th of August, 1768, was, for the most part, serene, and the afternoon was quite so. At near nine at night, the western horizon was illuminated with a very sensible twilight, which increased greatly upon that which the sun had left. I suspected that this quantity of light could be only owing to a beginning Aurora Borealis; and, accordingly, about ten o'clock, the sky being perfectly clear, excepting one thick cloud, about the same height as Arcturus, represented in my drawing, Tab. V. The Aurora was at that time considerable; several streams of light had then shot up from the horizon. At half an hour after ten, the Aurora occupied nearly one half of the horizon, extending from the west to the north-east, and the horizon seemed to be covered with an uneven thick smoak, from which issued several streamers of light; two of which, to the westward, arose to a great height, passing through the tail of the Great Bear, and were sensibly inclined to each other, tending to unite

 in


Ursa minor
Ursa major
North Line
Black Cloud
Arcturus
Auriga

in the zenith. Both theſe luminous ſtreamers kept in a continual agitation, which laſted the whole time of their exiſtence, that is, till eleven o'clock. At the foot of theſe lights was the furnace, which glowed with rays of light leſs elevated, and ſenſibly inclined to the horizon; theſe were alſo in continual agitation. At eleven, ſix ſtreamers, parallel to one another, ſhot up in the north, under the conſtellation of the Little Bear; they aſcended not ſo high, but were more conſpicuous than the two preceding ones, and their undulations were not ſo quick. About half an hour after eleven, the ſky began to be clouded; at midnight it was ſo all over, which put an end to all hope of further obſerving the progreſs of this phænomenon. In this Aurora I could not diſcern any lightning or rumbling, as I did in that which appeared in the night of the 21ſt of May, 1762, deſcribed in the fifth and ſixth volumes of the *Memoires des Sçavans Etrangers*.

Whilſt this phænomenon laſted, the air was calm, and no wind ſtirring; at leaſt what little there was, was from the north-eaſt. The quickſilver in the barometer ſtood at 28 inches $1\frac{1}{2}$ line; and Reaumur's thermometer at $20\frac{1}{4}$ degrees. During the whole day the barometer varied only $1\frac{1}{2}$ line, and the thermometer no more than five degrees.

An Account of an Aurora Borealis, *obſerved at* Paris, *the 5th of* December, 1768. *By the ſame.*

About ſeven in the evening, the northern quarter was enlightened with an incipient Aurora Borealis, which increaſed gradually. At eleven at night it was very

very conſpicuous, numbers of luminous ſtreamers darting up from below the horizon, ſome of them reaching the zenith; but none of the ſtreamers laſted any conſiderable time, and their light was but feeble. Several whitiſh clouds appeared in the north; the furnace occupied a great part of the horizon. This phænomenon laſted almoſt the whole night.

The ſame Aurora was obſerved at Berlin, where it laſted from ſix in the evening till nine. It was alſo obſerved at Vienna, where it is ſaid that the needle of a compaſs loſt, during this phænomenon, its uſual direction, ſhifting at firſt two degrees eaſtward, and afterwards four degrees the contrary way; it was at the ſame time remarked that the electrical machine had acquired an uncommon degree of force.

Received April 20.

XVI. *Observations on the* Expectations *of Lives, the Increase of Mankind, the Influence of great Towns on Population, and particularly the State of* London *with respect to Healthfulness and Number of Inhabitants. In a Letter from Mr.* Richard Price, *F. R. S. to* Benjamin Franklin, *Esq; LL. D. and F. R. S.*

DEAR SIR,

Read April 27 and May 4, 1769.

I BEG leave to submit to your perusal the following observations. If you think them of any importance, I shall be obliged to you for communicating them to the Royal Society. You will find that the chief subject of them is the present state of the city of London, with respect to healthfulness and number of inhabitants, as far as it can be collected from the bills of mortality. This is a subject that has been considered by others; but the proper method of calculating from the bills has not, I think, been sufficiently explained.

No competent judgment can be formed of the following observations, without a clear notion of what

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the writers on *Life Annuities* and *Reverſions* have called the *Expectation of Life*. Perhaps this is not in common properly underſtood; and Mr. De Moivre's manner of expreſſing himſelf about it is very liable to be miſtaken.

The moſt obvious ſenſe of the *expectation* of a given life is, "That particular number of years "which a life of a given age has an equal chance "of enjoying." This is properly the time that a perſon may reaſonably *expect* to live; for the chances *againſt* his living longer are greater than thoſe *for* it; and, therefore, he cannot entertain an *expectation* of living longer, conſiſtently with probability. This period does not coincide with what the writers on Annuities call the *expectation of life*, except on the ſuppoſition of an uniform decreaſe in the probabilities of life, as Mr. Simpſon has obſerved in his *Select Exerciſes*, p. 273.——It is neceſſary to add, that, even on this ſuppoſition, it does not coincide with what is called the *expectation of life* in any caſe of joint lives. Thus, two joint lives of 40 have an even chance, according to Mr. De Moivre's hypotheſis *, of conti-

* Mr. De Moivre's hypotheſis, here referred to, ſuppoſes (as is well known to thoſe who have ſtudied the ſubject of Life Annuities) an equal decrement of human life through all its ſtages. That is, it ſuppoſes that out of any given number alive at a given age, the ſame number will die every year till they are all dead. Thus; 86 Mr. De Moivre makes the utmoſt probable extent of life. The number of years which any given life wants of 86 he calls the *complement* of that life.——56, therefore, is the *complement* of 30; and ſuppoſing 56 perſons alive at this age, *one* will die every year till, in 56 years, they will be all dead. The like will happen to 46 at 40, to 36 at 50, and ſo on, for all other ages. This is an

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nuing together only $13\frac{1}{2}$ years. But the *expectation* of two equal joint lives being (according to the same hypothesis) always a *third* of the *common complement*, it is in this case $15\frac{1}{3}$ years. It is necessary, therefore, to observe, that there is another sense of this phrase which ought to be carefully distinguished from that now mentioned. It may signify "The *mean conti-*" "*nuance* of any given *single*, *joint*, or *surviving* lives, "according to any given table of observations:" that is, the number of years which, taking them one with another, they actually enjoy, and may be considered as sure of enjoying, those who live or survive *beyond* that period, enjoying as much *more* time in proportion to their number, as those who *fall short* of it enjoy *less*. Thus, Supposing 46 persons alive, all 40 years of age, and that, according to Mr. De Moivre's *hypothesis*, one will die every year till they are all dead in 46 years, half 46 or 23 will be their *expectation of life*: that is; The number of years enjoyed by them all will be just the same as if every one of them had lived 23 years, and then died; so that, supposing no interest of money, there would be no difference in value between annuities payable for life to every single person in such a set, and equal annuities payable to another equal set of persons of the same common age, supposed to be all sure of living just 23 years and no more.

excellent *hypothesis*. It eases exceedingly the labour of calculating the values of lives. It is remarkably agreeable to Dr. Halley's Table of Observations; and, as far as it implies an equal decrement of life, is, in a great measure, confirmed by other Tables.

In like manner; the *third* of 46 years, or 15 years and 4 months, is the *expectation* of two joint lives both 40; and this is alſo the *expectation* of the ſurvivor. That is; ſuppoſing a ſet of marriages between perſons all 40, they will, one with another, laſt juſt this time, and the ſurvivors will laſt the ſame time; and annuities payable during the continuance of ſuch marriages would, ſuppoſing no intereſt of money, be of exactly the ſame value with annuities to begin at the extinction of ſuch marriages, and to be paid, during life, to the ſurvivors. In adding together the years which any great number of ſuch marriages and their ſurvivorſhips have laſted, the ſums would be found to be equal.

One is naturally led to underſtand the *expectation* of life in the firſt of the ſenſes now explained, when, by Mr. Simpſon and Mr. De Moivre, it is called, *the number of years which, upon an equality of chance, a perſon may expect to enjoy*; or, *the time which a perſon of a given age may juſtly expect to continue in being*; and, in the laſt ſenſe, when it is called, *the ſhare of life due to a perſon* *. But, as in reality it is always uſed in the laſt of theſe ſenſes, the former language ſhould not be applied to it: and it is in this laſt ſenſe that it coincides with the *ſums* of the *preſent* probabilities that any given ſingle or joint lives ſhall attain to the end of the 1ſt, 2d, 3d, &c. *moments* from this time to the end of their poſſible exiſtence; or, in the caſe of ſurvivorſhips, with the ſum of the probabilities that

* See Mr. De Moivre on *Annuities*, p. 65, &c. 4th edition, and Mr. Simpſon's Select Exerciſes, p. 255, 273.

there

there ſhall be a ſurvivor at the end of the 1ſt, 2d, 3d, &c. *moments*, from this time to the end of the poſſible exiſtence of ſurvivorſhip. This coincidence every one converſant in theſe ſubjects muſt ſee, upon reflecting, that both theſe ſenſes give the true preſent value of a life-annuity ſecured by land, without intereſt of money *.

* The *ſum* of the probabilities that any given lives will attain to the end of the 1ſt, 2d, 3d, &c. *years* from the preſent time to the utmoſt extremity of life (for inſtance, $\frac{45}{46}+\frac{44}{46}+\frac{43}{46}$, &c. to $\frac{1}{46}=22\frac{1}{2}$ for lives of 40, by the *hypotheſis*) may be called their *expectation*, or the number of payments due to them, as *yearly annuitants*. The ſum of the probabilities that they will attain to the end of the 1ſt, 2d, 3d, &c. *half years* (or, in the particular caſe ſpecified, $\frac{91}{92}+\frac{90}{92}+\frac{89}{92}+\frac{88}{92}$, &c. $=\frac{91}{2}$ *half* years, or $22\frac{3}{4}$ *years*) is their expectation as *half yearly annuitants*. And the ſums juſt mentioned of the probabilities of their attaining to the end of the 1ſt, 2d, 3d, &c. *moments* (equal in the ſame particular caſe to 23 years) is properly their *expectation of life*, or their *expectation* as annuitants ſecured by land.

Mr. De Moivre has concealed the demonſtrations of the rules he has given for finding theſe *expectations* of life, and only intimated, in general, that he diſcovered them by a calculation deduced from the method of fluxions, p. 66, of his *Treatiſe on Annuities*. It will, perhaps, be agreeable to ſome to ſee how eaſily they are deduced in this method upon the hypotheſis of an equal decrement of life.

Let $\dot{x}$ ſtand for a moment of time and n the *complement* of any aſſigned life. Then $\frac{n-\dot{x}}{n}$, $\frac{n-2\dot{x}}{n}$, $\frac{n-3\dot{x}}{n}$, &c. will be the *preſent* probabilities of its continuing to the end of the 1ſt, 2d, 3d, &c. moments; and $\frac{n-x}{n}$ the probability of its continuing to the end of x time. $\frac{n-x}{n}\times\dot{x}$ will therefore be the *fluxion* of the ſum of the probabilities, or of an *area* repreſenting this ſum, whoſe

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This period in *joint* lives, I have obſerved, is *never*

ordinates are $\frac{n-x}{n}$, and *axis* x.——The *fluent* of this expreſſion, or $x-\frac{x^2}{2n}$ is the ſum itſelf for the time x; and this, when $x=n$, becomes $\frac{1}{2}n$, and gives the *expectation* of the aſſigned life, or the ſum of all the probabilities juſt mentioned for its whole poſſible duration.——In like manner: Since $\frac{\overline{n-x}^2}{n^2}$ is the probability that two equal joint lives will continue x time $\frac{n-x}{n^2}$ $\times\,\dot{x}$ will be the *fluxion* of the ſum of the probabilities. The *fluent* is $x-\frac{x^2}{n}+\frac{x^3}{3n^2}$, which when $n=x$ is $\frac{n}{3}$ the expectation of two equal joint lives.———Again: Since $\frac{n-x}{n}\times\frac{2x}{n}$ is the probability that there will be a ſurvivor of two equal joint lives at the end of x time, $\frac{n-x}{n}\times\frac{2x}{n}\times\dot{x}$ will be the *fluxion* of the ſum of the probabilities; and the *fluent*, or $\frac{x^2}{n}-\frac{2x^3}{3x^2}$ is (when $x=n$) $\frac{1}{3}n$, or the *expectation* of ſurvivorſhip between two equal lives, which therefore appears to be equal to the *expectation* of their joint continuance. The expectation of two *unequal* joint lives found in the ſame way is $\frac{m}{2}-\frac{m^2}{6n}$, m being the *complement* of the oldeſt life, and n the *complement* of the youngeſt. The whole *expectation* of ſurvivorſhip is $\frac{n}{2}-\frac{m}{2}+\frac{m^2}{3n}$. The expectation of ſurvivorſhip on the part of the oldeſt is, $\frac{m^2}{6n}$; and the expectation on the part of the youngeſt is, $\frac{n}{2}-\frac{m}{2}+\frac{m^2}{6n}$. It is eaſy to apply this inveſtigation to any number of joint lives, and to all caſes of ſurvivorſhip.

I have above endeavoured to ſhew diſtinctly how the *expectations* of *ſingle* lives may be found, agreeably to any Table of Obſervations, without having recourſe to any principles, except ſuch as are plain and common.

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the ſame with the period which they have an equal chance of enjoying; and in ſingle lives, I have obſerved, they are the ſame only on the ſuppoſition of an uniform decreaſe in the probabilities of life. If this decreaſe, inſtead of being always uniform, is *accelerated* in the laſt ſtages of life, the former period, in ſingle lives, will be *leſs* than the latter; if *retarded*, it will be *greater*.

It is neceſſary to add, that the number expreſſing the former period, multiplied by the number of ſingle or joint lives whoſe expectation it is added annually to a ſociety or town, gives the whole number living together, to which ſuch an annual addition would in time grow. Thus; ſince 19, or the third of 57, is the *expectation* of two joint lives whoſe common age is 29, or common *complement* 57, twenty marriages every year between perſons of this age would, in 57 years, grow to 20 times 19, or 380 marriages always exiſting together. The number of *ſurvivors* alſo ariſing from theſe marriages, and always living together, would, in twice 57 years, increaſe to the ſame number. And, ſince the *expectation* of a ſingle life is always half its *complement*, in 57 years likewiſe 20 ſingle perſons aged 29, added annually to a town, would increaſe to 20 times 28.5 or 570; and when arrived at this number, the deaths every year will juſt equal the acceſſions, and no further increaſe be poſſible.

It appears from hence, that the particular proportion that becomes extinct every year, out of the whole number conſtantly exiſting together of ſingle or joint lives, muſt, wherever this number undergoes no variation,

variation, be exactly the same with the *expectation* of those lives at the time when their existence commenced. Thus; was it found that a 19th part of all the marriages among any body of men, whose numbers do not vary, are dissolved every year by the deaths of either the husband or wife, it would appear that 19 was, at the time they were contracted, the *expectation* of these marriages. In like manner; was it found in a society, limited to a fixed number of members, that a 28th part dies annually out of the whole number of members, it would appear that 28 was their common expectation of life at the time they entered. So likewise; were it found in any town or district, where the number of births and burials are equal, that a 20th or 30th part of the inhabitants die annually, it would appear that 20 or 30 was the *expectation* of a child just born in that town or district. These *expectations*, therefore, for all *single* lives, are easily found by a *Table of Observations*, shewing the number that die annually at all ages, out of a given number alive at those ages; and the general rule for this purpose is "to divide "the sum of all the living in the Table at the age "whose expectation is required, and at all greater "ages, by the sum of all that die annually at "that age, and above it; or, which is the same, by "the number in the Table of the living at that age; "and half subtracted from the quotient will be the "required *expectation*." Thus, in Dr. Halley's Table, the sum of all the living at 20 and upwards is 20,724. The number living at that age is 598; and the former number divided by the latter, and half

half unity * ſubtracted from the quotient, gives 34.15 for the *expectation* of 20. The expectation of the ſame life by Mr. *Simpſon's* Table, formed from the bills of mortality of London, is 28.9.

Theſe obſervations bring me to the principal point which I have had all along in view. They ſuggeſt to us an eaſy method of finding the number of inhabitants in a place from a *Table of Obſervations*, or the *bills of mortality* for that place, ſuppoſing the yearly births and burials equal. "Find by the "Table, in the way juſt deſcribed, the *expectation* "of an infant juſt born, and this, multiplied by the "number of yearly births, will be the number of "inhabitants." At *Breſlaw*, according to Dr. Halley's Table †, though half die under 16, and therefore an infant juſt born has an *equal chance* of living only 16 years, yet his *expectation*, found by the rule I have given, is near 28 years; and this, multiplied by 1238 the number born annually, gives 34,664,

* This ſubtraction is neceſſary, becauſe the *diviſor* ought to be made as much greater than the number dying annually given in the Table, as the *expectation*, with ½ unity added, is greater than the *expectation*, on account of the number that will die, in the courſe of the year, out of thoſe who are continually added, in order to preſerve the number of the living the ſame.

In other words: If we conceive the *recruit* neceſſary to ſupply the *waſte* of every year to be made always at the *end* of the year, the *dividend* ought to be the *medium* between the numbers living at the *beginning* and the *end* of the year; that is, it ought to be taken *leſs* than the ſum of the living in the Table at and above the given age, by *half* the number that die in the year; the effect of which *diminution* will be the ſame with the *ſubtraction* I have directed.

† Vid. Lowthorp's Abridgment of the Philoſophical Tranſactions, vol. III. p. 669.

the number of inhabitants. In like manner; it appears from Mr. Simpſon's Table, that, though an infant juſt born in London has not an *equal chance* of living 3 years, his *expectation* is 20 years; and this number, multiplied by the yearly births, would give the number of inhabitants in London, were the births and burials equal. The medium of the yearly births, for the laſt 10 years, has been 15,710. This number, multiplied by 20, is 314,200; which is the number of inhabitants that there would be in London, according to the bills, were the yearly burials no more than equal to the births: that is, were it to ſupport itſelf in its number of inhabitants without any ſupply from the country. But for the laſt 10 years, the burials have, at an average, been 22,956, and exceeded the chriſtenings 7,246. This is, therefore, at preſent, the yearly addition of people to London from other parts of the kingdom, by whom it is kept up. Suppoſe them to be all, one with another, perſons who have, when they remove to London, an *expectation* of life equal to 30 years. That is; ſuppoſe them to be all of the age of 18 or 20, a ſuppoſition certainly far beyond the truth. From hence will ariſe, according to what has been before obſerved, an addition of 30 multiplied by 7,246, that is 217,380 inhabitants. This number, added to the former, makes 531,580; and this, I think, at moſt, would be the number of inhabitants in London were the bills perfect. But it is certain that they give the number of births and burials too little. There are many burying-places that are never brought into the bills. Many alſo emigrate to the navy and army and country; and theſe ought to be added to the

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number of deaths. What the deficiencies arising from hence are, cannot be determined. Suppose them equivalent to 6000 every year in the births, and 6000 in the burials. This would make an addition of 20 times 6000 or 120,000 to the last number, and the whole number of inhabitants would be 651,580. If the burials are deficient only two thirds of this number, or 4000, and the births the whole of it; 20 multiplied by 6000, must be added to 314,290 on account of the defects in the births: and, since the excess of the burials above the births will then be only 5,246; 30 multiplied by 5,246 or 157,380, will be the number to be added on this account; and the sum, or number of inhabitants, will be 591,580. But if, on the contrary, the burials are deficient 6000, and the births only 4000; 80,000 must be added to 314,290, on account of the deficiencies in the births; and 30 multiplied by 9,246, on account of the excess of the burials above the births, and the whole number of inhabitants will be 671,580.

Every supposition in these calculations seems to me too high. *Emigrants* from London are, in particular, allowed the same *expectation* of continuance in London with those who are born in it, or who come to it in the firmest part of life, and never afterwards leave it; whereas it is not credible that the former *expectation* should be so much as half the latter. But I have a further reason for thinking that this calculation gives too high numbers, which has with me irresistible weight. It has been seen that the number of inhabitants comes out less on the supposition, that the defects in the christenings are greater

than thoſe in the burials. Now it ſeems evident that this is really the caſe; and, as it is a fact not attended to, I will here endeavour to explain diſtinctly the reaſon which proves it.

The proportion of the number of births in London, to the number who live to be 10 years of age, is, by the bills, 16 to 5. Any one may find this to be true, by ſubtracting the *annual medium* of thoſe who have died under 10, for ſome years paſt, from the *annual medium* of births for the ſame number of years.——Now, tho', without doubt, London is very fatal to children, yet it is incredible that it ſhould be ſo fatal as this implies. The *bills*, therefore, very probably, give the number of thoſe who die under 10 too great in proportion to the number of births; and there can be no other cauſe of this, than a greater deficiency in the *births* than in the *burials*. Were the deficiencies in both equal, that is, were the *burials*, in proportion to their number, juſt as deficient as the *births* are in proportion to *their* number, the proportion of thoſe who reach 10 years of age to the number born would be right in the *bills*, let the deficiencies themſelves be ever ſo conſiderable. On the contrary, were the deficiencies in the *burials* greater than in the *births*, this proportion would be given too great; and it is only when the former are leaſt that this proportion can be given too little.——Thus; let the number of annual *burials* be 23,000; of *births* 15,700; and the number dying annually under 10, 10,800. Then 4,900 will reach 10 of 15,700 born annually; that is, 5 out of 16.——Were there no deficiencies in the *burials*, and were it fact that only *half* die under 10, it would follow, that there was an annual

annual deficiency equal to 4,900 ſubtracted from 10,800, or 5,900 in the *births*.——Were the *births* a third part too little, and the *burials* alſo a third part too little, the true number of *births*, *burials*, and of *children dying under* 10, would be 20,933——30,666, and 14,400; and, therefore, the number that would live to 10 years of age would be 6,533 out of 20,933, or 5 of 16 as before.——Were the *births* a third part, and the *burials* ſo much as two-fifths wrong, the number of *births*, *burials*, and children dying under 10 would be 20,933——32,200 and 15,120; and, therefore, the number that would live to 10 would be 5,813 out of 20,933, or 5 out of 18——Were the *births* a 3d part wrong, and the *burials* but a 6th, the foregoing numbers would be 20,933——26,833——12,600; and, therefore, the number that would live to 10 would be 8,333 out of 20,933, or 5 out of 12.56: and this proportion ſeems as low as is conſiſtent with any degree of probability. It is ſomewhat leſs than the proportion in Mr. Simpſon's Table of *London Obſervations*, and near *one half* leſs than the proportion in the Table of *Obſervations* for Breſlaw, where it appears that above 9 of 16 live to be 10, and that *one half* live to be 16. The deficiencies, therefore, in the *births* cannot be much leſs than double thoſe in the *burials* *; and the leaſt numbers I have given

* One obvious reaſon of this fact is, that *none* of the *births* among *Jews*, *Quakers*, *Papiſts*, and the *three denominations of Diſſenters* are included in the bills, whereas *many* of their *burials* are. It is further to be attended to, that the abortive and ſtill-born, amounting to about 600 annually, are included in the burials, but never in the births. If we add theſe to the chriſten-

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muſt, probably, be neareſt to the true number of inhabitants. However, ſhould any one, after all, think that it is not improbable that only 5 of 16 ſhould live in London to be 10 years of age, or that above *two thirds* die under this age, the conſequence of admitting this will ſtill be, that the foregoing calculation has been carried too high. For it will from hence follow, that the *expectation* of a child juſt born in London cannot be ſo much as I have taken it. This *expectation* is 20, on the ſuppoſition that half die under 3 years of age, and that 5 of 16 live to be 29 years of age, agreeably to Mr. Simpſon's Table. But if it is indeed true, that *half* die under 2 years of age, and 5 of 16 under 10, agreeably to the *bills*, this expectation muſt be leſs than 20, and all the numbers before given will be conſiderably reduced.

Upon the whole: I am forced to conclude from theſe obſervations, that the ſecond number I have given, or 651,580, though ſhort of the number of inhabitants commonly ſuppoſed in London, is, very probably, *greater*, but cannot be much *leſs*, than the true number. Indeed, it is in general evident, that in caſes of this kind numbers are very much overrated. The ingenious Dr. Brakenridge *, 14 years ago, when the bills were lower than they are now, from the number of houſes, and allowing ſix to a houſe, made the number of inhabitants 751,800. But his method of determining the num-

ings, preſerving the burials the ſame, the proportion of the born, according to the bills, who have reached ten for the laſt ſixteen years, will be very nearly one *third* inſtead of *five ſixteenths*.

* Vid. Phil. Tranſact. vol. XLVIII.

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ber of houſes is too precarious; and, beſides, ſix to a houſe is, probably *, too large an allowance. Many families now have two houſes to live in. The magiſtrates of Norwich, in 1752, took an exact account of both the number of houſes and individuals in that city. † The number of houſes was 7,139, and of

* If this is true, Dr. Brakenridge has alſo over-rated the number of people in England. The number of houſes rated to the window tax he had, he ſays, been certainly informed was 690,000. The number of cottages not rated was not, he adds, accurately known; but from the accounts given in it appeared, that they could not amount to above 200,000; and, allowing 6 to a houſe, this would make the number of people in England 5,340,000. But if 5 to a houſe ſhould be a juſter allowance, the number will be 4,450,000. The number of people in Scotland he reckons 1,500,000, and in Ireland 1,000,000.—See a Letter to George Lewis Scott, Eſq; Phil. Tranſact. vol. XLIX. p. 877. 1756.

† Vid. Gentleman's Magazine for 1752, and Dr. Short's *Comparative hiſtory of the increaſe of mankind*, p. 38. In page 58 of this laſt work the author ſays, that, in order to be fully ſatisfied about the number of perſons to be allowed to a family, he procured the true number of families and individuals in 14 market towns, ſome of them conſiderable for trade and populouſneſs; and that in them were 20,371 families, and 97,611 individuals, or but little more than $4\frac{3}{4}$ to a family. He adds, that, in order to find the difference in this reſpect between towns of trade and country pariſhes, he procured from divers parts of the kingdom the exact number of *families* and *individuals* in 65 country pariſhes. The number of *families* was 17,208; *individuals* 76,284; or not quite $4\frac{1}{2}$ to a family.—In the place I have juſt referred to, in the Gentleman's Magazine, there is an account of the number of *houſes* and *inhabitants* in Oxford excluſive of the colleges, and in Wolverhampton, Coventry, and Birmingham, for 1750. The number of perſons to a houſe was, by this account, $4\frac{4}{5}$ in the two former towns, and $5\frac{3}{4}$ in the two latter. It ſeems, therefore, to appear that 5 perſons to a houſe is an allowance large enough for London, and too large for England in general.

indivi-

individuals 36,169, which gives nearly 5 to a houſe. ——Another method which Dr. Brakenridge took to determine the number of inhabitants in London was from the annual number of burials, adding 2000 to the bills for omiſſions, and ſuppoſing a 30th part to die every year. In order to prove this to be a moderate ſuppoſition he obſerves that, according to Dr. Halley's Obſervations, a 34th part die every year at Breſlaw. But this obſervation was made too inadvertently. The number of annual burials there, according to Dr. Halley's account, was 1174, and the number of inhabitants, as deduced by him from his Table, was 34,000, and therefore a 29th part died every year. Beſides; any one may find, that in reality the Table is conſtructed on the ſuppoſition, that the whole number born, or 1238, die every year; from whence it will follow that a 28th part died every year. * Dr. Brakenridge, therefore, had he attended to this, would have ſtated a 24th part as the proportion that dies in London every year, and this would have taken off 150,000 from the number he has given. But even this muſt be leſs than the juſt proportion. For let three fourths of all who either die in London or migrate from it, be ſuch as have been born in London; and let the reſt be perſons who have removed to London from the country or from foreign nations.

* Care ſhould be taken, in conſidering Dr. Halley's Table, not to take the firſt number in it, or 1000, for ſo many juſt born. 1238, he tells us, was the annual medium of births, and 1000 is the number he ſuppoſes all living at one year and under. It was inattention to this that led Dr. Brakenridge to his miſtake.

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The *expectation* of the former, it has been ſhewn, cannot exceed 20 years, and 30 years have been allowed to the latter. One with another, then, they will have an *expectation* of $22\frac{1}{2}$ years. That is, one of $22\frac{1}{2}$ will die every year. *And, conſequently,

* The whole number of inhabitants in Rome, in the year 1761, was 157,452; of whom 90,239 were males, and 67 213 females. And the annual medium of births, for 3 years from 1759 to 1761, was 5,167, and of burials 7,153. According to this account, therefore, a 22d part of the inhabitants die in Rome every year. See Dr. Short's *Comparative Hiſtory of the increaſe and decreaſe of mankind in England and ſeveral countries abroad*, p. 59, 60.——In Berlin, as the ſame author relates, p. 69, in ſix years, from 1734 to 1740, the annual medium of births was 3,504, of burials 3,639, and the number of inhabitants was 68,197; males 32,990, and females 35,207. A 19th part, therefore, of the inhabitants of Berlin are buried every year. As numbers taken by actual ſurvey are generally too little, ſuppoſe, in the preſent inſtance, an error committed in reckoning the number of inhabitants, equal to a 10th of the whole number, or to the whole number of children under 5; and ſuppoſe likewiſe no omiſſions in the burials. The conſequence will be, that about 1 in 21 are buried at Berlin every year.——At Dublin, in the year 1695, the number of inhabitants was found, by an exact ſurvey, to be 40,508 (ſee Philoſ. Tranſactions, N° 261). I find no account of the annual burials juſt at that time; but from 1661 to 1681, the medium had been 1613; and from 1715 to 1728 it was 2123. There can, therefore, be no material error in ſuppoſing that in 1695 it was 1800; and this makes 1 in 22 to die annually.——In 1745 the number of *families* in the ſame city appeared, by an exact account laid before the Lord Mayor, to be 9,214. It is probable, this number of families did not conſiſt of more than 50,000 individuals. Suppoſe them, however, 55,000; and, as at this time the medium of annual burials appears to have been 2,360; 1 in 23 died annually: ſee Dr. Short's *Comparative Hiſtory*, p. 15, and *New Obſervations*, p. 228.——I know not how far theſe facts may be depended on. If they come at all near the truth, they demonſtrate that I have been very moderate in making

ſuppoſing the annual recruit from the country to be 7000, the number of *births* 3 times 7000 or 21,000, and the *burials* and *migrations* 28,000 (which ſeem to be all high ſuppoſitions), the number of inhabitants will be $22\frac{1}{2}$ multiplied by 28,000, or 630,000.

I will juſt mention here one other inſtance of exaggeration on the preſent ſubject.

Mr. Corbyn Morris, in his *Obſervations on the paſt*

only 1 in $22\frac{1}{2}$, including emigrants, to die in London annually. ——In 1631 the number of people in the *city and liberties* of London was taken, by order of the Privy Council, and found to be 130,178.——This account was taken five years after a plague that had ſwept off near a quarter of the inhabitants; and when, therefore, the town being full of recruits in the vigour of life, the medium of annual burials muſt have been lower than uſual, and the births higher. Could, therefore, the medium of annual burials at that time, within the walls and in the 16 pariſhes without the walls, be ſettled, excluſive of thoſe who died in ſuch parts of the 16 pariſhes without the walls, as are not in the *liberties*, the proportion dying annually obtained from hence might be depended on, as rather leſs than the common and juſt proportion. But this medium cannot be diſcovered with any accuracy. *Graunt* eſtimates that two thirds of theſe 16 pariſhes are within the *liberties*; and, if this is right, the medium of annual burials in the *city and liberties* in 1631, was 5,500, and 1 in $23\frac{3}{4}$ died annually; or, making a ſmall allowance for deficiencies in the bills, 1 in 22.——Mr. Maitland, in his Hiſtory of London, vol. II. p. 744, by a laborious, but too unſatisfactory, inveſtigation, reduces this proportion to 1 in $24\frac{1}{2}$; and on the ſuppoſitions, that this is the true proportion dying annually, *at all times*, in London, and that the deficiencies in the burials amount to 3,038 annually, he determines that the number of inhabitants within the bills was 725,903 in the year 1737.

The number of burials not brought to account in the bills is, probably, now much greater than either Dr. Brakenridge or Mr. Maitland ſuppoſe it. I have reckoned it ſo high as 6000, in order to include emigrants, and alſo to be more ſure of not falling below the truth.

growth

growth and preſent ſtate of the city of London, publiſhed in 1751, ſuppoſes that no more than a 60th part of the inhabitants of London, who are above 20, die every year, and from hence he determines that the number of inhabitants was near a million. In this ſuppoſition there was an error of at leaſt one half. According to Dr. Halley's Table, it has been ſhewn, that a 34th part of all at 20 and upwards, die every year at Breſlaw. In London, a 29th part, according to Mr. Simpſon's Table, and alſo according to all other Tables of London Obſervations. And in *Scotland* it has been found for many years, that of 974 miniſters and profeſſors whoſe ages are 27 and upwards, a 33d part have died every year. Had, therefore, Mr. Morris ſtated a 30th part of all above 20 as dying annually in London, he would have gone beyond the truth, and his concluſion would have been 400,000 leſs than it is.

Dr. Brakenridge obſerved, that the number of inhabitants, at the time he calculated, was 127,000 leſs than it had been. The bills have lately advanced, but ſtill they are much below what they were from 1717 to 1743. The medium of the annual *births*, for 20 years, from 1716 to 1736, was 18,000, and of *burials* 26,529; and by calculating from hence on all the ſame ſuppoſitions with thoſe which made 651,580 to be the preſent number of inhabitants in London, it will be found that the number then was 735,840, or 84,260 greater than the number at preſent. London, therefore, for the laſt 30 years, has been decreaſing; and though now it is increaſing again, yet there is reaſon to think that the additions lately made to the number of

 buildings

buildings round it, are owing, in a great meaſure, to the increaſe of luxury, and the inhabitants requiring more room to live upon*.

It ſhould be remembered, that the number of inhabitants in London is now ſo much leſs as I have made it, than it was 40 years ago, on the ſuppoſition that the proportion of the omiſſions in the *births* to thoſe in the *burials* was the ſame then that it is now. But it appears that this is not the fact.——From 1728, the year when the ages of the dead was firſt given in the *bills*, to 1742, near five-ſixths of thoſe who were born died under 10, according to the *bills*.——From 1742 to 1752 three quarters; and ever ſince 1752 this proportion has ſtood nearly as it is now, or at ſomewhat more than two-thirds. The omiſſions in the *births*, therefore, compared with thoſe in the *burials* were greater formerly; and this muſt render the difference between the number of inhabitants now and formerly leſs conſiderable than it may ſeem to be from the face of the bills. One reaſon why the proportion of the amounts of the *births* and *burials* in the bills comes now nearer than it did to

* The medium of annual burials in the 97 pariſhes within the walls was,

From 1655 to 1664,	——	3264
From 1680 to 1690,	——	3139
From 1730 to 1740,	——	2316
From 1758 to 1768,	——	1620

This account proves, that though, ſince 1655, London has doubled its inhabitants, yet, *within the walls*, they have decreaſed; and ſo rapidly for the laſt 30 years as to be now reduced to one half.——The like may be obſerved of the 17 pariſhes immediately without the walls. Since 1730 theſe pariſhes have been decreaſing ſo faſt, that the *annual burials* in

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the true proportion, may, perhaps, be that the number of Diſſenters is conſiderably leſſened. The Foundling Hoſpital alſo may have contributed a little to this event, by leſſening the number given in the bills as having died under 10, without taking off any from the *births*; for all that die in this hoſpital are buried at *Pancraſs* church, which is not within the *bills*. See the preface to a collection of the yearly bills of mortality from 1657 to 1758 incluſive, p. 15.

I will add, that it is probable that London is now become leſs fatal to children than it was; and that this is a further circumſtance which muſt reduce the difference I have mentioned; and which is likewiſe neceſſary to be joined to the greater deficiencies in the births, in order to account for the very ſmall proportion of children who ſurvived 10 years of age, during the two firſt of the periods I have ſpecified. Since 1752, London has been thrown more open. The cuſtom of keeping country-houſes, and of ſending children to be nurſed in the country, has prevailed more. But, particularly, the deſtructive uſe of ſpirituous liquors among the poor has been checked.

I have ſhewn that in London, even in its preſent ſtate,

them have ſunk from 8,672 to 5,432, and are now lower than they were before the year 1660. In Weſtminſter, on the contrary, and the 23 out-pariſhes in Middleſex and Surrey, the *annual burials* have, ſince 1660, advanced from about 4000 to 16,000.——Theſe facts prove that the inhabitants of London are now much leſs crowded together than they were. It appears, in particular, that *within the walls* the inhabitants take as much room to live upon as double their number did formerly.——The very ſame concluſions may be drawn from an examination of the *chriſtenings*.

ſtate, and according to the moſt moderate computation, half the number born die under *three* years of age; and I have obſerved that at Breſlaw half live to 16. At Edinburgh, if I may judge from ſuch of its bills as I have ſeen, almoſt as great a proportion of children die as even in London. But it appears from *Graunt's* * accurate account of the births, weddings, and burials in three country pariſhes for 90 years; and alſo, with abundant evidence, from Dr. Short's collection of obſervations in his *Comparative Hiſtory*, and his treatiſe entitled, *New Obſervations on Town and Country Bills of Mortality* †; that in country villages and pariſhes, the major part live to mature age, and even to marry. So great is the difference, eſpecially to children, between living in great towns and in the country. But nothing can place this obſervation in a more ſtriking light than the curious account given by Dr. Thomas Heberden, and publiſhed in the Philoſophical Tranſactions

* See *Natural and Political Obſervations on the Bills of Mortality*, by Captain John Graunt, F. R. S.

† The public is much obliged to this author for the pains he has taken in collecting obſervations on the mortality and increaſe of mankind, in different countries and ſituations. In his New Obſervations, p. 309, he mentions an ingenious pariſh clerk, in the country, who, by a particular account which he took, found that of 314, who had been baptized in his pariſh in one year, 80, or nearly a quarter part, died under four years of age. Forty-ſix died the firſt year; thirteen the ſecond; ſixteen the third; and five the fourth. After four, life grows more ſtable, and at ten acquires its greateſt ſtability; and in this caſe it cannot be reckoned that above a 10th, or, at moſt, an 8th more than the quarter that died under four, would die under age; and therefore, probably, near two-thirds arrived at maturity.

actions (vol. LVII. p. 461), *of the increaſe and mortality of the inhabitants of the iſland of Madeira.* In this iſland, it ſeems, the weddings have been to the births, for 8 years, from 1759 to 1766, as 10 to 46.8; and to the burials as 10 to 27.5. Double theſe proportions, therefore, or the proportion of 20 to 46.8, and of 20 to 27.5 are the proportions of the number marrying annually, to the number born and the number dying. Let 1 marriage in 10 be a 2d or 3d marriage on the ſide of either the man or the woman, and 10 marriages will imply 19 individuals who have grown up to maturity, and lived to marry once or oftener; and the proportion of the number marrying annually the firſt time, to the number dying annually, will be 19 to 27.5, or near 3 to 4. It may ſeem to follow from hence, that in this iſland near three-fourths of thoſe who die have been married, and, conſequently, that not many more than a *quarter* of the inhabitants die in childhood and celibacy; and this would be a juſt concluſion were there no increaſe, or had the births and burials been equal. But it muſt be remembered, that the general effect of an increaſe, while it is going on in a country, is to render the proportion of perſons marrying annually to the annual deaths *greater*, and to the annual births *leſs* than the true proportion marrying out of any given number born. This proportion generally lies between the other two proportions, but always neareſt to the firſt *; and, in the preſent caſe, it is ſufficiently evident that it cannot be much leſs than two-thirds.

* In a country where there is no increaſe, or decreaſe of the inhabitants, and where alſo life, in its firſt periods, is ſo ſtable,

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In London, then, *half* die under three years of age, and in Madeira about *two-thirds* of all who are

and marriage ſo much encouraged, as that half all who are born live to be married, the *annual* births and burials muſt be equal, and alſo *quadruple* the number of weddings, after allowing for 2d and 3d marriages. Suppoſe in theſe circumſtances (every thing elſe remaining the ſame) the *probabilities of life*, during its firſt ſtages, to be improved. In this caſe, more than *half* the born will live to be married, and an increaſe will take place. The births will exceed the burials, and both fall below *quadruple* the weddings; or, which is the ſame, below *double* the number annually married.——Suppoſe next (the *probabilities of life* and the *encouragement to marriage* remaining the ſame) the *prolifickneſs* only of the marriages to be improved. In this caſe it is plain, that an increaſe alſo will take place; but the *annual* births and burials, inſtead of being leſs, will now both riſe above *quadruple* the weddings, and therefore the proportion of the born to that part of the born who marry (being by ſuppoſition two to one) will be leſs than the proportion of either the *annual* births or the *annual* burials to the number marrying *annually*.——Suppoſe again (the *encouragement to marriage* remaining the ſame) that the *probabilities of life* and the *prolifickneſs of marriages* are both improved. In this caſe, a more rapid increaſe will take place, or a greater exceſs of the births above the burials; but at the ſame time they will keep nearer to *quadruple* the weddings, than if the latter cauſe only had operated, and produced the ſame increaſe.——I ſhould be too minute and tedious, were I to explain theſe obſervations at large. It follows from them, that, in every country or ſituation where, for a courſe of years, the *burials* have been either *equal to* or *leſs* than the *births*, and both under *quadruple* the marriages; and alſo that wherever the burials are *leſs* than quadruple the annual marriages, and at the ſame time the births *greater*, there the major part of all that are born live to marry. In the inſtance which I have conſidered above, and which occaſions this note, the annual births are ſo much *greater* than *quadruple* the marriages, and at the ſame time the annual burials ſo much *leſs*, that the proportion that lives to marry of thoſe who are born can ſcarcely be much leſs than I have ſaid, or two-thirds.

born live to be married. Agreeably to this, it appears alſo from the account I have referred to, that the *expectation* of a child juſt born in Madeira is about 39 years, or near double the expectation of a child juſt born in London. For the number of inhabitants was found, by a ſurvey made in the beginning of the year 1767, to be 64,614. The annual medium of *burials* had been, for eight years, 1293; of *births* 2201. The number of inhabitants, divided by the annual medium of *burials*, gives 49.89, or the *expectation* nearly of a child juſt born, ſuppoſing the *births* had been 1293, and conſtantly equal to the *burials*, the number of inhabitants remaining the ſame. And the ſame number, divided by the annual

I have ſhewn how the allowance is to be made for 2d and 3d marriages; but it is not ſo conſiderable as to be of any particular conſequence; and, beſides, it is, in part, compenſated by the natural children which are included in the births, and which raiſe the proportion of the births to the weddings higher than it ought to be, and therefore bring it nearer to the true proportion of the number born *annually*, to thoſe who marry annually, after deducting thoſe who marry a 2d or 3d time.

In drawing concluſions from the proportion of *annual* births and burials in different ſituations, ſome writers on the increaſe of mankind have not given due attention to the difference in theſe proportions ariſing from the different circumſtances of increaſe or decreaſe among a people. One inſtance of this I have now mentioned; and one further inſtance of it is neceſſary to be mentioned. The proportion of *annual* births to weddings has been conſidered as giving the true number of children derived from each marriage, taking all marriages one with another. But this is true only when, for many years, the births and burials have kept nearly equal. Where there is an exceſs of the births occaſioning an increaſe, the proportion of *annual* births to weddings muſt be leſs than the proportion of children derived from each marriage; and the contrary muſt take place where there is a decreaſe.

medium of *births*, gives 29.35, or the *expectation* of a child just born, supposing the burials 2201, the number of births and of inhabitants remaining the same; and the true *expectation* of life must be somewhere near the mean between 49.89 and 29.35.

Again: A 50th part of the inhabitants of Madeira, it appears, die annually. In London, I have shewn, that above twice this proportion dies annually. In smaller towns a smaller proportion dies, and the births also come nearer to the burials. At Breslaw, I have observed, that, by Dr. Halley's Table, a 28th part dies annually; and the annual medium of births, for a complete century, from 1633 to 1734, has been 1089; of burials 1256. * At Norwich, the annual medium of births, dissenters included, for four years, from 1751 to 1754, was 1150; of burials 1214. And as the number of inhabitants was at that time 36,169 (see pag. 103), a 30th part of the inhabitants died annually. In general, there seems reason to think that in towns (allowing for particular advantages of situation, trade, police, cleanliness, and openness, which some towns may have), the excess of the burials above the births and the annual deaths are more or less as the towns are greater or smaller. In London itself, about 160 years ago, when it was scarcely a fourth part of its present bulk, the births were nearly equal to the burials.

* Vid. Dr. Short's *Comparative History*, p. 63. And the *Abridgment of the Philosophical Transactions*, vol. VII. part iv. p. 46. During the five years on which Dr. Halley has founded his Table, or from 1687 to 1691, the births happened a little to exceed the burials.

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But in country pariſhes and villages the births almoſt always exceed the burials; and I believe it ſeldom happens that ſo many as a 30th, or much more than a 40th part of the inhabitants die annually *. In the four provinces of New England there is a very rapid increaſe of the inhabitants: but, notwithſtanding this, at Boſton, the capital, the inhabitants would decreaſe were there no ſupply from the country: for, if the account I have ſeen is juſt, from 1731 to 1762, the burials have all along exceeded the births †. So remarkably do towns, in conſequence of their unfavourableneſs to health, and the luxury which generally prevails in them, check the increaſe of countries.

* In 1738 there was an account taken of the number of families and inhabitants in the Pruſſian dominions. The number of inhabitants was 2,138,465. The medium of annual births, weddings, and burials was nearly 84,000; 21,000, and 55,481. Near a 40th part, therefore, died every year. Vid. Dr. Snort's *Comparative Hiſtory*, p. 69, and *Abridgment of the Philoſophical Tranſactions*, ibid.——The proportion of weddings and burials to the births ſhews that, in theſe countries, there was a quick increaſe, notwithſtanding the waſte in the cities.——In the year 1733 a ſurvey was taken of the inhabitants of the pariſh of *Stoke Damerel* in *Devonſhire*, and the number of men, women, and children, was found to be 3361.——The *chriſtenings* for the year were 122——the *weddings* 28——*burials* 62.——No more, therefore, than the 54th part of the inhabitants died in the year.—— In part of this year an epidemical fever prevailed in the pariſh. See Martyn's *Abridgment of the Philoſ. Tranſactions*, vol. IX. p. 325.—— According to Graunt's account of a pariſh in *Hampſhire*, not reckoned, he ſays, remarkably healthful, a 50th part of the inhabitants had died annually for 90 years. *Natural and Political Obſervations*, &c. Chap. xii.

† See a particular account of the births and burials in this town from 1731 to 1752 in the *Gentleman's Magazine* for 1753, p. 413.

Healthfulneſs and Prolifickneſs are, probably, cauſes of increaſe ſeldom ſeparated. In conformity to this obſervation, it appears from comparing the births and weddings, in countries and towns where regiſters of them have been kept, that in the former, marriages, one with another, ſeldom produce leſs than four children each; generally between four and five, and ſometimes above five. But in towns ſeldom above four; generally between three and four; and ſometimes under three *.

I have ſometimes heard the great number of old people in London mentioned to prove its favourableneſs to health and long life. But no obſervation can be much more erroneous. There ought, in reality, to be more old people in London, in proportion to the number of inhabitants, than in any ſmaller towns, becauſe at leaſt one quarter of its inhabitants are perſons who come into it, from the country, in the moſt robuſt part of life, and with a much greater probability of attaining old age, than if they had come into it in the weakneſs of infancy. But, notwithſtanding this advantage, there are much fewer perſons who attain to great ages in London than in any other place where obſervations have been made.—— At Vienna, of 22,704 who died in the four years

* Any one may ſee what evidence there is for this, by conſulting the accounts in Dr. Short's two books already quoted; and in the *Abridgment of the Philoſophical Tranſactions*, vol. VII. part iv. p. 46.—In conſidering theſe accounts, it ſhould not be forgotten that allowances muſt be made for the different circumſtances of increaſe or decreaſe in a place, agreeably to the obſervation at the end of the note in pag. 113.

1717,

1717, 1718, 1724, 1725*, 109 reached 90 years, that is, 48 in 10,000. But in London, for the laſt 30 years, only 35 of the ſame number have reached this age.——At Breſlaw it appears, by Dr. Halley's Table, that 41 of 1238 born, or a 30th part, live to be 80 years of age.——In the pariſh of *All-ſaints* in Northampton†, an account has been kept for many years of the ages at which all die; and, I find, that of 1377, who died there in 13 years, 59 have lived to be 80, or a 23d part.——According to Mr. Kerſſeboom's Table of Obſervations, publiſhed at the end of the laſt edition of Mr. De Moivre's Treatiſe on the Doctrine of Chances, a 14th part of all that are born live to be 80; and, had we any obſervations in *country* pariſhes, this, probably, would not appear to be too high a proportion‡. But in London, for the laſt 30 years, only 25 of every 1000

* Vid. Abridgment of the Philoſophical Tranſactions, vol. VII. part iv. p. 46.——It appears alſo that more than three-fifths of all who died in theſe years at Vienna were boys and girls, by whom, I ſuppoſe, are meant perſons under 16. About the ſame proportion dies under 16 at Berlin.

† In this town, as in moſt other towns of any magnitude, the births, including Diſſenters, fall ſhort of the burials; and the greater part die under age.

‡ This, however, will appear itſelf inconſiderable, when compared with the following account: "In 1761, the burials in "the diſtrict of Chriſtiana, in Norway, amounted to 6,929, and "the chriſtenings to 11,024. Among thoſe who died, 394, or "1 in 18, had lived to the age of 90; 63 to the age of 100, and "ſeven to the age of 101.——In the dioceſe of Bergen, the per- "ſons who died amounted only to 2,580, of whom 18 lived to "the age of 100; one woman to the age of 104, and another "woman to the age of 108."

See the *Annual Regiſter* for 1761, p. 191.

who

who have died, have lived to be 80, or a 40th part; which may be eaſily diſcovered by dividing the ſum of all who have died during theſe years at all ages, by the ſum of all who have died above 80.

Among the peculiar evils to which great towns are ſubject, I might further mention the PLAGUE. Before the year 1666 this dreadful calamity laid London almoſt waſte once in every 15 or 20 years; and there is no reaſon to think that it was not generally bred within itſelf. A moſt happy alteration has taken place, which, perhaps, in part, is owing to the greater advantages of cleanlineſs and openneſs, which London has enjoyed ſince it was rebuilt, and which lately have been very wiſely improved.

The facts I have now taken notice of are ſo important that, I think, they deſerve more attention than has been hitherto beſtowed upon them. Every one knows that the ſtrength of a ſtate conſiſts in the number of people. The encouragement of population, therefore, ought to be one of the firſt objects of policy in every ſtate; and ſome of the worſt enemies of population are the luxury, the licentiouſneſs, and debility produced and propagated by great towns.

I have obſerved that London is now* increaſing. But it appears that, in truth, this is an event more to be dreaded than deſired. The more London in-

* This increaſe is greater than the bills ſhew, on account of the omiſſion in them of the two pariſhes which have been moſt encreaſed by new buildings; I mean *Marybone* and *Pancraſs* pariſhes. The former of theſe pariſhes is, I ſuppoſe, now one of the largeſt in London.

creaſes,

creaſes, the more the reſt of the kingdom muſt be deſerted; the fewer hands muſt be left for agriculture; and, conſequently, the leſs muſt be the plenty and the higher the price of all the means of ſubſiſtence. ——*Moderate* towns, being ſeats of refinement, emulation, and arts, may be public advantages. But *great* towns, long before they grow to half the bulk of London, become checks on population of too hurtful a nature, nurſeries of debauchery and voluptuouſneſs; and, in many reſpects, greater evils than can be compenſated by any advantages*.

* The mean annual *births*, *weddings*, and *burials* in the following towns, for ſome years before 1768, were nearly,

	Births.	Weddings.	Burials.
At Paris,	19,200	4.300	19,500
Vienna,	5,600		6,800
Amſterdam,	4,500	2,400	7,600
Copenhagen,	2,700	868	3,100

In the Paris bills there is, I am informed, an omiſſion of all that die in the *Foundling Hoſpital*, amounting to above 2000 annually. The exceſs, therefore, of the burials above the births is greater than the bills ſhew. This exceſs, however, is much leſs than could have been expected in ſo large a town. I am not ſure to what cauſe this ought to be aſcribed; but I cannot wonder at it, if it be indeed true, that a fifth of all born in Paris are ſent to the *Foundling Hoſpital*, and that a third of the inhabitants die in *hoſpitals*, and alſo that all married men are excuſed from ſerving in the militia, from whence draughts are made for the army. Theſe are encouragements to marriage and population, which no other city enjoys; and it is ſtrange that in this kingdom ſome policy of the ſame kind with that laſt mentioned ſhould not be purſued.——A further ſingularity in the ſtate of Paris is, that the births in it are above four times the weddings, nothing like which is the caſe in any other town whoſe bills I have ſeen. It may ſeem, therefore, that here, as well as in the moſt healthful

Dr. Heberden

Dr. Heberden obſerves that, in Madeira, the inhabitants double their own number in 84 years. But

and increaſing country pariſhes, each marriage produces more than four children; but this is a concluſion which, in the preſent caſe, cannot be depended on. It ſhould be conſidered that, probably, ſome who leave the country to ſettle at Paris, come to it already married; and that no ſmall proportion of the births may be illegitimate. Theſe cauſes, however, may only balance the allowance to be made for the ſecond and third marriages among the annual weddings; and, if it is indeed fact, that the people at Paris are ſo prolific as they appear to be in the bills, it will only prove more ſtrongly that, like other great towns, it is very unfavourable to health; for the more prolific a people are, the greater muſt be the mortality among them if they do not increaſe. ——Let us ſuppoſe the true number of deaths at Paris, including emigrants and ſuch as die in the *Foundling Hoſpital*, to be 21,000; the number married annually 2 × 4,300 or 8,6000; and the births, as before, 19,200. 1,900 then will be the number of annual recruits from the country. Of theſe let only 1,200 be ſuppoſed to marry: and 8,600 leſſened by 1,200, or 7,400, will be the number of thoſe born at Paris who marry annually; and 11,800, or above *three-fifths* will be the number dying in childhood and celibacy. This, though it gives an unfavourable repreſentation of Paris when compared with the country, makes it appear to advantage when compared with ſome other great towns. I am not ſufficiently informed of the ſtate of Paris to know how near this calculation comes to the truth. Every ſuch doubt would be removed, were the ages of the dead given in the Paris bills. It is much to be wiſhed this was done. The births and burials here come ſo near to one another, that there can ſcarcely be a properer place for ſuch bills; and a Table of Obſervations might be formed from them that would give the values of lives much more exactly than the London Tables.

I cannot help adding that, excepting the omiſſion I have mentioned in the burials, the Paris bills are complete; but it is well known that the London bills are extremely otherwiſe. London, therefore, muſt be much larger in compariſon of Paris than it appears to be in the bills.

this

this (as you, Sir, well know) is a very ſlow increaſe compared with that which takes place among our colonies in AMERICA. In the back ſettlements, where the inhabitants apply themſelves entirely to agriculture, and luxury is not known, they double their own number in 15 years; and all through the northern colonies in 25 years*. This is an inſtance of increaſe ſo rapid as to have ſcarcely any parallel. The births in theſe countries muſt exceed the burials much more than in Madeira, and a greater proportion of the born muſt reach maturity.——In 1738, the number of inhabitants in New Jerſey was taken by order of the government, and found to be 47,369. Seven years afterwards the number of inhabitants was again taken, and found to be increaſed, by procreation only, above 14,000, and very near one *half* of the inhabitants were found to be under †16 years of age. In 22 years, therefore, they muſt have doubled their own number, and the births muſt have exceeded the burials 2000 annually. As the increaſe here is much quicker than in Madeira, we may be ſure that a ſmaller proportion of the inhabitants muſt die annually. Let us, however, ſuppoſe it the ſame, or a 50th part. This will make the annual burials

* See a diſcourſe on *Chriſtian union*, by Dr. Styles, Boſton, 1761, p. 103. 109, &c.——See alſo *The intereſt of Great Britain conſidered with regard to her Colonies, together with Obſervations concerning the increaſe of mankind, peopling of countries, &c.* p. 35. 2d edit. London, 1761.

† According to Dr. Halley's Table the number of the living under 16 is but a *third* of all the living at all ages; and this may be nearly the caſe in all places which juſt ſupport themſelves in the number of their inhabitants, and neither increaſe or decreaſe.

 to

to have been, during these seven years, 1000, and the annual births 3000, or an 18th part of the inhabitants.——Similar observations may be made on the much quicker increase in Rhode Island, as related in the preface to Dr. Birch's *collection of the bills of mortality*, and also in the valuable pamphlet, last quoted, on *the interest of Great Britain with regard to her colonies*, p. 36.——What a prodigious difference must there be between the vigour and the happiness of human life in such situations, and in such a place as London?——The original number of persons who, in 1643, had settled in New England, was 21,200. Ever since it is reckoned, that more have left them than have gone to them*. In the year 1760 they were increased to half a million. They have, therefore, all along doubled their own number in 25 years; and, if they continue to increase at the same rate, they will, 70 years hence, in New England alone, be four millions; and in all North America above twice the number of inhabitants in Great-Britain†.——But I am wandering

* See Dr. Styles's pamphlet just quoted, p. 110, &c.

† The rate of increase, supposing the procreative powers the same, depends on two causes: The "encouragement to marriage;" and the "*expectation* of a child just born." When one of these is given, the increase will be always in proportion to the other. That is; As much *greater* or *less* as the *ratio* is of the numbers who reach maturity, and of those who marry to the number born, so much *quicker* or *slower* will be the increase.——Let us suppose the operation of these causes such as to produce an annual excess of the *births* above the *burials* equal to a 36th part of the whole number of inhabitants. It may seem to follow from hence, that the inhabitants would double their own number in 36 years; and thus some have calculated. But the truth is, that they would double their own number in much less time.

from

from my purpose in this letter. The point I had chiefly in view was, the present state of London as

Every addition to the number of inhabitants from the births produces a proportionably greater number of births, and a greater excess of these above the burials; and if we suppose the excess to increase annually at the same rate with the inhabitants, or so as to preserve the *ratio* of it to the number of inhabitants always the same, and call this *ratio* $\frac{1}{r}$, the period of doubling will be the *quotient* produced by dividing the logarithm of 2 by the *difference* between the logarithms of $r+1$ and r, as might be easily demonstrated. In the present case, r being 36, and $r+1$ being 37, the period of doubling comes out 25 years. If r is taken equal to 22, the period of doubling will be 15 years.—— But it is certain that this ratio may, in many situations, be greater than $\frac{1}{22}$; and, instead of remaining the same, or becoming less, it may *increase*, the consequence of which will be, that the period of doubling will be shorter than this rule gives it.——According to Dr. Halley's Table, the number of persons between 20 and 42 years of age is a third part of the whole number living at all ages. The prolific part, therefore, of a country may very well be a 4th of the whole number of inhabitants; and supposing four of these, or every other marriage between persons all under 42, to produce *one* birth every year, the annual number of births will be a 16th part of the whole number of people; and, therefore, supposing the burials to be a 48th part, the annual excess of the births above the burials will be a 24th part, and the period of doubling 17 years.——The number of inhabitants in New England was, as I have said from Dr. Stiles's pamphlet, half a million in 1760. If they have gone on increasing at the same rate ever since, they must be now 640,000; and it seems to appear that in fact they are now more than this number. For, since I have writ the above observations, I have seen a particular account, grounded chiefly on surveys lately taken with a view to taxation and for other purposes, of the number of males, between 16 and 60, in the four provinces. According to this account, the number of such males is 218,000. The whole number of people, therefore, between 16 and 60, supposing 14 males to 13 females, must be nearly

to healthfulneſs, number of inhabitants, and its influence on population. The obſervations I have made may, perhaps, help to ſhew how the moſt is to be made of the lights afforded by the London bills, and ſerve as a ſpecimen of the proper method of calculating from them. It is indeed extremely to be wiſhed that they were leſs imperfect than they are, and extended further. More pariſhes round London might be taken into them; and, by an eaſy improvement in the pariſh regiſters now kept, they might be

420,000. In order to be more ſure of avoiding exceſs, I will call them only 400,000. In Dr. Halley's Table the proportion of all the living under 16 and above 60, to the reſt of the living, is 13.33 to 20; and this will make the number of people now living in the four provinces of New England to be 666,000. But, on account of the rapid increaſe, this proportion muſt be conſiderably greater in New England, than that given by Dr. Halley's Table. In New Jerſey, I have ſaid the number of people under 16 was found to be almoſt equal to the number above 16. Suppoſe, however, that in New England, where the increaſe is ſomewhat ſlower, the proportion I have mentioned is only 16 to 20, and then the whole number of people will be 720,000.

I cannot conclude this note without adding a remark to remove an objection which may occur to ſome in reading Dr. Heberden's account of Madeira, to which I have referred. In that account 5945 is given as the number of children under ſeven in the iſland, at the beginning of the year 1767. The medium of annual births, for eight years, had been 2201; of burials 1293. In ſix years, therefore, 13,206 muſt have been born; and if, at the end of ſix years, no more than 5945 of theſe were alive, 1210 muſt have died every year. That is; almoſt all the burials in the iſland, for ſix years, muſt have been burials of children under ſeven years of age. This is plainly incredible; and, therefore, it ſeems certain, that the number of children under ſeven years of age muſt, through ſome miſtake, be given, in that account, 3000 or 4000 too little.

extended

extended through all the pariſhes and towns in the kingdom. The advantages ariſing from hence would be very conſiderable. It would give the preciſe law according to which human life waſtes in its different ſtages, and thus ſupply the neceſſary *data* for computing accurately the values of all *life-annuities* and *reverſions*. It would, likewiſe, ſhew the different degrees of healthfulneſs of different ſituations, mark the progreſs of population from year to year, keep always in view the number of people in the kingdom, and, in many other reſpects, furniſh inſtruction of the greateſt importance to the ſtate. Mr. De Moivre, at the end of his book on the doctrine of chances, has recommended a general regulation of this kind; and obſerved, particularly, that at leaſt it is to be wiſhed, that an account was taken, at proper intervals, of all the living in the kingdom, with their ages and occupations; which would, in ſome degree, anſwer moſt of the purpoſes I have mentioned.——But, dear Sir, I am ſenſible it is high time to finiſh theſe remarks. I have been carried in them far beyond the limits I at firſt intended. I always think with pleaſure and gratitude of your friendſhip. The world owes to you many important diſcoveries; and your name muſt live as long as there is any knowledge of philoſophy among mankind. That your happineſs in this, and every other reſpect, may continually increaſe, is the ſincere wiſh of,

Sir,

Your much obliged,
and very humble ſervant,

Newington-Green,
April 3, 1769.

Richard Price.

XVII. *Diſſertatio*

XVII. *Diſſertatio epiſtolaris de Oſſibus et Dentibus Elephantum, aliarumque Belluarum in* America Septentrionali, *aliiſque borealibus Regionibus obviis; qua indigenarum Belluarum eſſe oſtenditur. Auctore* R. E. Raſpe, *Sereniſſimo* Haſſiarum *Landgravio à Conſiliis, et R. S. S.*

SOCIETATI REGIÆ SCIENTIARUM LONDINENSI.

Read May 11, 1769.

PERSCRIPSIT ad me illuſtris Pringlius, baronettus: " Media nu-
" per in America Septentrionali, non procul ab
" Ohio fluvio, intra ſpatium duorum aut trium
" jugerum terræ paludoſæ, ſale fœcundæ, ſceleta
" triginta belluarum majorum inventa fuiſſe, nulla
" aut rara terra ſolum ſepulta: Si magnitudinis
" oſſium atque dentium majorum habeatur ratio, ele-
" phantina videri; ob formam molarium dentium,
" qui carnivori potius ſint animalis, ambigi cui bel-
" luarum ſpeciei tribuenda; quæri inter naturæ ſtu-
" dioſos de phænomeni cauſis; pro ſua in me bene-
" volentia et meam rogare ſententiam."

Hanc diſſertationem, quamvis difficultates quæſtionis magis oſtendat quam ſolvat, malui judicio veſtro potius

potius ſubjicere quam dare aliud quodvis ſtudiorum meorum documentum.

Duplex proprie eſt quæſtio; altera, "de cauſa, "quæ harum belluarum oſſa in Americam Septen-"trionalem tranſvexerit?" Altera, "quæ in tam "brevi ſpatio concluſerit et ſepelierit?"

Primam ſoluturo occurrit:

1. Hiſtoricorum Americæ conſtans obſervatio, omnem hanc novi orbis terram, cum tribus fere abhinc ſeculis detecta Europæis patere cœpit, nonniſi à quadrupedibus minoribus fuiſſe habitatam; peregrina igitur videri illa, ſive elephantum ſive aliarum belluarum coloſſalium ſint oſſa.

2. In mentem redeunt oſſa majora in Aſiæ aut Sibiriæ borealibus regionibus inventa et obvia, quæ ſubterraneo lucifugo animali, quod Mammout dicitur, tribuit fabula, elephantis aliiſque belluis vindicant commentarii Petropolitani et Gmelinus in Itinerario Sibiriaco. Nec in Aſiæ et Americæ ſolum borealibus provinciis ea occurrunt, effodiuntur etiam in Europæ climatibus ſeptentrionalibus.

De Britannicis huc pertinentia quædam in Tranſactionibus veſtris legiſſe memini. De Suecia fidem faciunt Holmienſis academiæ commentarii. Ex Germaniæ naturali hiſtoria minus vulgatas aliquot obſervationes addere liceat.

Nil moror gigantum oſſa, quæ credula antiquitas ſpectanda ſervavit in cathedralibus quibuſdam templis, ut Halberſtadenſi, Ganderſtæmenſi, aliiſque. Huc facere quidem videntur, ſed certiora repeto et primo quidem ex electoratus et ducatus Brunſvicenſis naturali hiſtoria, diligentius olim à me perſcrutata. Recordemini oſſium in antris Baumanniano, Schartzfeldenſi, et prope Dünam

nam in Hercynia effoſſorum. Ea ſcriptis eruditorum Leibnitzii in *Protogæa* et Hollmanni in Commentariis Goettingenſibus celebrata ſunt, atque cuilibet inſpectanti belluarum majorum terreſtrium eſſe videntur, ex parte rhinocerotum oſſibus perſimilia ab Hollmanno demonſtrata ſunt. Dentem majorem, elephantinis forma et ſubſtantiæ textura exacte convenientem et ad Fiſchbeckam prope Hamelam ad Viſurgim inventum, quem Hannoveræ in inſtructiſſimo ſuo muſeo ſervat mihi amiciſſimus Andreæ, plus una vice manibus verſavi. Vidi etiam Blumenaviæ prope Hannoveram effoſſam majorem coſtam incerti animalis. De ſpecie belluæ majoris ad Quedlinburgum effoſſæ, cujus meminit Guerickius Magdeburgenſis conſul in libro *de vacuo*, quam maxime anceps eſt quæſtio. Certius pronunciare licet de dentibus, in fundamentis jaciendis urbis Neo-Caſſellenſis, quam nunc inhabito et per Sereniſſimi Landgravii Haſſiarum in me cumulatam et liberalitatem et gratiam alteram prædico patriam, effoſſis. Iſti elephantini ſunt; et cum aliis naturæ et artis miraculis in muſeo publico, mihi ex parte concredito, ſervantur. Plura non addo exempla, non quod deficiat materies, nam et antiquitas Græca et Romana et quicquid fere eſt cultiorum populorum ſimilia adnotavit; adducta vero ſufficiunt ad probandum "in omnibus borealibus regionibus et antiqui et novi orbis inveniri majorum belluarum oſſa, quæ vivæ in illis non ſuperſunt, et per cœli inclementiam nunquam in illis generaſſe videntur."

3. Subit accuratius perpendendus ſitus et natura ſoli in quibus deprehendi ſolent.

In

In ſtratis lapideis, corporibus marinis mixta, incluſa, tecta inveniri nuſquam legi.

Deliteſcant in cavernis montium ut in tribus illis Hercyniæ antris; aut palude, aut terra hortenſi, aut limo exterioris ſuperficiei terræ vix contecta atque involuta jacent, quod de Americanis, Sibiriacis, aliiſque, ex relationibus fide dignis, conſtat.

Nec etiam, ut corpora marina ſaxis incluſa, lapidis duritiem et naturam induerunt. Diverſimode calcinata, parum aut nihil mutata ſunt.

In Europæ ſeptentrionalibus plagis rarius jam et ſolitaria ſere deteguntur, forte quod exhauſta curioſitatis materies, et quod diutius habitatæ a populis miraculorum et medicamentorum appetentibus, nam oſſa hujuſmodi, eboris aut unicornu foſſilis nomine, in officinis pharmacopœorum proſtant, et neſcio quam illis antidoti mirabilem vim tribuerat antiquitas.

In ipſis quoque Americæ et Sibiriæ terris, habita ratione marinorum corporum in lapidem verſorum immenſæ copiæ, rariora ſunt; et minor utique belluarum terreſtrium progenies in natali earum terra, quaſi ſecus non ſuffectura, obſervata fuit.

Quæ omnia ſi recte animo colligantur duplicem eamque ad rem facientem ſuſpicionem movent; alteram "alio plane modo hæc oſſa ſepulta eſſe, quam "quæ profundius in lapidibus latent corpora mari-"na; alteram non eſſe peregrinas et marinas belluas "haſce, quarum ſunt partes, ſed fato ſuo, aut ho-"minum vi, illas in ipſis hiſce borealibus terris, "olim illis natalibus, exſtinctas fuiſſe."

Quæſtio igitur propoſita, jamque ſolvenda, eſt de generali quadam cauſa, quæ has belluas, aut ſub ſole tantum

tantum calente degentes, in Septentrionem transtulerit, aut exstinxerit similes illis species olim Septentrionis incolas.

Si peregrinas alioque advectas et cœli solùm calidioris progeniem statuamus, omnis definitio periculosa videtur.

Terræ motuum et Vulcanorum vis, quæ insulas produxit et montes, et rationem sufficientem reddit stratorum lapideorum, variè inclinatorum et ruptorum, illisque inclusorum marinorum corporum, nil valet ad advehendas orientis et meridiei bestias. Superficiem terræ sundique marium mutat. Climata inconcussa manent.

Diluvii universalis Noachici aut Deucalionei cujusdam effectibus, præter alia multa, quod supra notavi, obstat, ossa hæc vel in cavernis montium, solitis ferarum latebris, aut paludibus aut superiore solum terra, solitaria quadammodo, nunquam corporibus marinis tecta, mixta, inclusa deprehendi. Nec etiam, quæcunque tam mirabili aquarum super omnem continentem effusioni tribuatur vis et potentia, concipi animo potest, quare ossa et cadavera hæc graviora ex meridionalibus regionibus in septentrionales transvexerit, eaque in alta potius mediaque continente terra deposuerit, quam in oceani abyssis, nisi forte cum Woodwardo perplaceat diluvii tempore nullam omnino gravitatis fuisse actionem, quod quidem per me licet, neminem vero vestrum rapiet in assensum.

Si recurramus ad antiquitatis remotissimæ heroas, belluis uti belli instrumentis utentes, easque invitas cum militibus suis æque forte invitis in ultimam Europam, Sibiriam, atque Americam vi, metuque propellentes, quæstionem non solvimus sed impedimus.

America enim toto oceano ab antiquo orbe divisa, et quicquid dicant veteres, navalis eorum scientia exercitibus cum elephantis in Americam ex Asia aut Europa transvehendis non sufficit. Quatenus etiam lux est in antiquorum temporum historia et moribus, sero quadrupedes in bellum domarunt, elephantos primò nonnisi Asiæ et Africæ reges. Tenebris vero historiarum, quæ maximam antiqui orbis partem premunt, ad fabulas novas fingendas meo quidem judicio abuti non licet, nisi forte de quibuscunque historiæ et orbis terris incognitis impune mentiri liceat.

Peregrinas igitur non esse nec aliò advectas has Septentrionis belluarum majorum relliquias quammaxime prona est conjectura; idque et illi sensere, qui climata terræ mutata, eoque ipso omnem controversiam definiri posse putarunt. Vereor autem ne eorum hypotheses, nimio autorum ingenio, collapsuræ sint.

Climata terræ, eodem orbitæ, qua circum solem revolvitur, diametro servato, duplici modo mutari atque affici posse videntur; aut magis imminuta istius orbitæ sive eclíptices obliquitate magisque ad planum æquatoris inclinata; aut axeos terræ centrique gravitatis sumta quadam mutatione, quæ cum climatibus simul et terræ figuram mutat.

Prior hypothesis mirè arrisit recentioribus quibusdam philosophis, tum quod problema propositum apparenter solvat, tum etiam quod talis quædam orbitæ vel ecliptices inclinatio ejusque obliquitatis diminutio revera accidit, si quidem Eratosthenis atque Hipparchi, novemdecim ab hinc seculis institutis, observationibus sua fides constat. Illi eclipticam ab æquatoris plano suo tempore 23° 51′ 20″ recedere nota-

runt, quam celeberrimus De la Caille noſtris diebus $22' \, 59''\frac{1}{2}$ minorem obſervavit in Promontorio Bonæ Spei, uti quidem notavit in *Lect. Elem. Aſtron.* Nolo contra antiquarum obſervationum fidem diſputare, nec repetere quæ de incertitudine annuarum legum hujus imminutæ ecliptices obliquitatis et de cauſa ejus non perpetua, inter aſtronomos diſputata ſunt. Concedam potius multo majorem olim fuiſſe ecliptices obliquitatem; ipſa vero mea in hujus hypotheſeos patronos liberalitate vincam. Quamcunque enim majorem orbitæ obliquitatem fingere velint, nunquam tamen eò evitabunt neceſſariò inde provenientes tempeſtatum anni viciſſitudines, naturæ elephantorum quammaxime contrarias. Extendant tropicos ad quinquageſimum et ultra latitudinis gradum; intendant quantum velint calorem æſtivum, et certe magis intenſus fiet majore orbitæ obliquitate, et magis directe ſolis radiis in zonas, quas inhabitamus, incidentibus, quid elephantis, quibus illa hypotheſi in noſtris regionibus commodas, et vivendi et generandi ſedes parare ſatagunt, inde commodi? Non equidem video: ſentio potius, idque ſphæroïdea terræ figura ipſaque major ecliptices obliquitas cuilibet perpendenti facile perſuadebit, majorem hunc æſtivum calorem, conſtante ſemeſtri viciſſitudine, majus etiam frigus hibernum inſecuturum, quod tantum abeſt ut elephantorum naturali progeniei in zonis borealibus conveniat ut potius illi quammaxime obſtet.

Quod ad axeos terræ centrique gravitatis mutationem attinet, ea aut momentanea eſſe debuiſſet, aut lenta, et per gradus ſucceſſiva.

Si prius; hypotheſis cœlum Acherontaque movet, ſumma miſcet imis, omnemque hominum et animalium

lium viviparorum progeniem fato ſubitæ mortis, et ſine miraculo inevitabilis involvit, in perpetuum interituram et ſine novo creationis miraculo non reviviſcendam; ejuſmodi enim mutatio, quæ meridionales zonas olim inter tropicos ſitas uno temporis momento boreæ objiciat, ſine univerſali terræ ruina exitiali cogitari nequit, ſi, quod veriſſimum demonſtravit Newtonus, rotatio orbis polos deprimit æquatorem pandit, diameterque terræ per polos ductus minor ſit altero æquatoris ſeptem et quod excurrit milliaribus Germanicis. "Quis talia fando temperet à lacrymis!" Tam violenta etiam climatum mutatio boreæ olim incolarum cadavera ſub æquatorem in meridionales terras tranſvehere debuiſſet, quod an experientia confirmetur viderint alii quibus hæc arrident.

Sed lento gradu procedere potuit illa figuræ terræ, climatum et axeos mutatio? Concedam. Sed quare non amplius procedit? Quis impedit ne, certo quodam et procedenti axeos mutationi reſpondente progreſſu, meridionales plagas cum terræ trepidatione et mugitu verſus boream procedentes paullatim deprimat et mergat, boreales vero ad meridiem jam tendentes extollat oceano? Conſtantes ſibi fingamus naturæ leges aut nullas. Non enim ſeneſcit.

Redeo igitur ad illam, quam ſupra jam indigitavi, ſuſpicionem, ſuo fato, aut hominum vi, obiiſſe belluas, quarum oſſa nos exercent, ubi deprehendimus, nec peregrinas eſſe, nec marinas. Ea ſola minoribus difficultatibus laborat. Quamvis enim nec climata, nec axem terræ, nec ejus figuram mutandam propterea cenſeam, ſunt tamen quædam quæ obſtare illi videntur, quo minus pro generali aliqua phænomeni

propoſiti

propoſiti cauſa haberi poſſit; ſed obſtare tantummodo videntur.

Maximam difficultatem hoc parit, pro elephantinis aut meridionalium belluarum oſſibus haberi; Septentrionem et omnem Americam elephantos ſimiliſque voluminis animalia non generare; et ſi animalium et climatum natura conſtans ſit, et perpetua, nunquam generaſſe.

Totius igitur quæſtionis cardo verſatur circa naturam horum oſſium: an reverà elephantina aut talia ſint, quæ animali in ſeptentrione degenti convenire non poſſint? Et mihi quidem plus quam indeciſa videtur illa oſſium natura; certè elephanto vindicari non poſſunt molares dentes, ſimul cum reliquis in America eodem loco inventi; nec etiam alia oſſa in Sibiria et Hercyniæ cavernis obvia illi conveniunt, quod accuratius perlegenti commentarios Petropolitanos et Gmelini itinerarium, et ſine præconcepta opinione inſpectanti oſſa, ſat clarum erit. Si per anatomiam omnium quadrupedum imprimis belluarum inter ſe comparatam, habito reſpectu ad diverſas illarum ætates, quæ oſſium formam mutant, conſtaret, nullum cujuſcunque animalis os alteri convenire et convenire poſſe, quod non conſtat; tum quidem coactus confiterer re verâ elephantina eſſe quæ videntur, reliqua quæ elephantina non videntur adſcribenda ſpeciei nondum nec in India nec alibi terrarum obſervatæ. Quod an difficultates omnes expediat judicate ipſi; ne enim repetam cum nulla hypotheſi elephantorum meridionalium ſub ſeptentrione oſſa conciliari poſſe, anne admittenda erit alia belluarum ſpecies nondum obſervata, cui molares dentes elephantis

non

non convenientes adſcribantur? Species forte nullibi ſuperſtes, ſi quidem nobis datum eſt omnes noſſe, omnes ritè deſcripſiſſe? Exſtirpata fortè aut emortua? Aut etiam neſcio in quo mundi angulo, philoſopho nulli viſo, adhuc deliteſcens?

Quæ cum ita ſint, licere mihi puto omnia hæc ſeptentrionis coloſſalia oſſa uni ſolum ſpeciei incognitæ, olim ſeptentrionis incolæ, aut adſcribere etiam diverſis. Tum me reliquarum hypotheſium omnium defectus non vexabunt, et magnis me difficultatibus me explicuiſſe putabo——" forte ut incidam in Scyl-" lam volens evitare Charybdim," quod veſtrum judicium eſto.

Integras ferarum ſpecies in Britannia, Germania, aliiſque Europæ provinciis penitus exſtirpatas, aut quam proxime emorituras ipſi noſtis, aut quod nocivæ habitæ fuerint, aut in pellibus et carnibus earum luxuriatum ſit genus humanum. De lupis, urſis, alce, uro, pleriſque, Cæſaris ævo, in Germania Rhenana obviis ſermo mihi eſt. Quidni igitur noxiam aliquam belluæ ſpeciem, rarius generantem per longam ſeculorum ſeriem exſcindere potuiſſet ſeptentrionalium populorum ab omni hiſtoriæ memoria celebratum venationis et bellicæ fortitudinis ſtudium? Certe ea via ad gloriam ibant omnes, et Herculem et quemvis primi ſeculi heroa acrem monſtrorum debellatorem cantat fabula. Caſtorum et zibellinorum ſpeciebus ſimile quid noſtris ferè diebus accidit, eoque minus de almæ naturæ lege, ſpecies omnes ſervante, contra me diſputari poteſt; et in ſeculo quidem pluribus animantium ſpeciebus fatali, et ab homine in omnium ferè ſpecierum individua ſævo. Species etiam in ſeptentrione non ſuperſtites ſupereſſe poſſunt in zonis antarcticis.

antarcticis nostris borealibus parallelis. Quis negat? quis novit?

Elephantorum Africæ, Indiæ, et Ceylanensium diversitatem quandam notant naturæ historici, Ceylanenses reliquis omnibus magnitudine et ingenio præcellere prædicantes. Esse igitur potuit elephantorum species in ipso septentrione, reliquis omnibus præcellens frigoris patientia, et à meridionalibus elephantis non magis diversa quam Lappones aut Hurones diversi esse solent a Nigritis et Malabariæ incolis. Sed certi quid pronunciare non ausim. Sufficit, omnium reliquarum hypothesium insufficientiam, naturam situs et ossium ipsorum, tandem etiam exempla exstirpatarum specierum persuadere, quammaximè probabili quadam ratione, belluas quarum ossa in septentrione hinc inde latent, non esse peregrinas, non aliò advectas sed indigenas in illis terris, ipsis olim natalibus, vixisse, generasse, quamvis hodie non supersint.

Altera jam quæstio de causa, quæ tam ingentem sceletorum atque ossium acervum in tam brevi Americæ Septentrionalis spatio concluserit et sepelierit? quammaxime est expedita. Latebant in terra paludosa, sale fœcunda, rara solum terra tecta; non quod hæ belluæ de salinarum, dominio et proprietate humana quadam arte et crudelitate, invicem debellarint et fortitudine sua ceciderint in fundo ad prœliandum quammaxime incommodo, quod videri poterat primo obtutu et jocabundo cuidam amicorum meorum videbatur; sed quod salis liguriendi deliciis in paludem allectæ, paludosa terra ponderi corporis cedente, haustæ, impeditæ, sepultæ fuerunt, luxuriem et voluptatem morte luentes.

Hæc

Hæc fere ſunt quæ ad ſolvendas aut impediendas ſupra nominatas quæſtiones mihi quidem dici poſſe videntur, elegantius forte et doctius dicenda, ſi magis verborum limatorum illecebris munerumque pretio atque pondere quam dantis et ſcribentis animo deliniendi et capiendi foretis. Illum vero vobis addictiſſimum teſtor, ſincera vota pro veſtræ Societatis incolumitate et flore fundentem, veſtræque ſalutis et veritatis omnis cupidiſſimum.

R. E. Raſpe.

XVIII. *Obſervations on a particular Manner of Increaſe in the Animalcula of vegetable Infuſions, with the Diſcovery of an indiſſoluble Salt ariſing from Hempſeed put into Water till it becomes putrid. By* John Ellis, *Eſq; F. R. S.*

Read May 28, 1769.

HAVING, at the requeſt of Dr. Linnæus, made ſeveral experiments on the infuſion of muſhrooms in water, in order to prove the theory of Baron Munchhauſen, that their ſeeds are firſt animals and then plants; which he takes notice of in his Syſtem of Nature, p. 1326, under the genus of *Chaos*, by the name of *Chaos fungorum ſeminum*: it appeared evidently that the ſeeds were put into motion by very minute animalcula which proceeded from the putrefaction of the muſhroom; for by pecking at theſe ſeeds, which are reddiſh, light, round bodies, they moved them about with great agility in a variety of directions, while the little animals themſelves were ſcarcely viſible, till the food they had eaten had diſcovered them. The ſatisfaction I received from clearing up this point led me into many other curious and intereſting experiments. I looked carefully over Mr. Turbervill Needham's, F. R. S. very ingenious memoir on this ſubject, vol. XLV. p 615, of the Philoſophical Tranſactions:

Tranſactions: I mean as to the experiments, many of which ſucceeded with me, ſome not*. I own, his

* The ingenious Mr. Needham ſuppoſes, thoſe little tranſparent ramified filaments, and jointed or coralloid bodies, which the microſcope diſcovers to us on the ſurface of moſt animal and vegetable infuſions when they become putrid, to be zoophytes or branched animals: but to me they appear (after a careful ſcrutiny with the beſt glaſſes) to be of that claſs of *Fungi* called *Mucor* or Mouldineſs, many of which Michelius has figured, and Linnæus has accurately deſcribed.

Their vegetation is ſo amazingly quick, that they may be perceived in the microſcope, even to grow and feed under the eye of the obſerver.

Mr. Needham has pointed out to us one that is very remarkable for its parts of fructification. See Phil. Tranſ. vol. XLV. Tab. v. fig. 3, *a*, A; this, he ſays, proceeded from an infuſion of bruiſed wheat.

I have ſeen the ſame ſpecies ariſe from the body of a dead fly, which was become putrid by lying floating for ſome time in a glaſs of water, where ſome flowers had been, in the month of Auguſt, 1768. This ſpecies of *Mucor* ſends forth a maſs of tranſparent filamentous roots, from whence ariſe hollow ſtems, that ſupport little oblong-oval ſeed veſſels with a hole on the top of each; from theſe I could plainly ſee minute globular ſeeds iſſue forth, in great abundance, with an elaſtic force, and turn about in the water as if they were animated.

Continuing to view them with ſome attention, I could juſt diſcover, that the putrid water, which ſurrounded them, was full of the minuteſt animalcula, and that theſe little creatures began to attack the ſeeds of the *Mucor* for food, as I have obſerved before in the experiment on the ſeeds of the larger kind of *Fungi* or muſhrooms. This new motion continued the appearance of their being alive for ſome time longer: but ſoon after many of them aroſe to the ſurface of the water, remaining there without motion; and a ſucceſſion of them afterwards coming up, they united together in little thin maſſes, and floated to the edge of the water, remaining there, quite inactive during the time of obſervation.

As this diſcovery had cleared up many doubts, which I had conceived from reading Mr. Needham's learned diſſertation, I

reaſoning is very ſpecious and plauſible, but too metaphyſical for a natural hiſtorian. Yet I cannot forbear relating one of the experiments which I tried in conſequence of his diſcovery, that animalcula were produced in various infuſions, notwithſtanding the greateſt heat was given to the liquor.

On the 25th of May, 1768, Fahrenheit's thermometer 70 degrees, I boiled a potatoe in New-river

put into the ſame glaſs ſeveral other dead flies, by which means this ſpecies of *Mucor* was propagated ſo plentifully, as to give me an opportunity of frequently trying the ſame experiment to my full ſatisfaction.

Laſtly, thoſe jointed coralloid bodies, which Mr. Needham calls chaplets and pearl necklaces, I have ſeen frequently very diſtinctly. Theſe appear not only on an infuſion of bruiſed wheat, when it becomes putrid, but on moſt other bodies, that throw up a viſcid ſcum, and are in a ſtate of putrefaction. Theſe then are evidently no more than the moſt common *Mucor*, the ſeeds of which are every where floating in the air; and bodies in this ſtate afford them a proper and natural ſoil to grow upon. Here they ſend downwards their fine tranſparent ramified roots into the moiſture which they float upon, and from the upper part of the ſcum their jointed coralloid branches riſe full of ſeed into little grove-like figures. When a ſmall portion of theſe branches and ſeeds are put into a drop of the ſame putrid water the ſcum floats upon, many of the millions of little animalcula, with which it abounds, immediately ſeize them as food, and turn them about with a variety of motions; as in the experiment on the ſeeds of the common muſhrooms; either ſingly or two or three ſeeds connected together, anſwering exactly to Mr. Needham's deſcription; but evidently without any motion of their own, and conſequently not animated.

I am ſatisfied Mr. Needham's obſervations have convinced him long before this, that they muſt be vegetables: for my part, I own I have never ſeen a zoophyte extend its branches, and grow out of water. I hope I have already cleared up that point, in ſhewing the abſurdity of Dr. Pallas's *Corallina terreſtris*, Phil. Tranſ. vol. LVII. p. 425.

water till it was reduced to a mealy confiftence. I put part of it, with an equal proportion of the boiling liquor, into a cylindrical glafs veffel that held fomething lefs than half a wine pint, and covered it clofe immediately with a glafs cover. At the fame time, I fliced an unboiled potatoe, and, as near as I could judge, put the fame quantity into a glafs veffel of the fame kind, with the fame proportion of New-river water, not boiled, and covered it with a glafs cover, and placed both veffels clofe to each other.

On the 26th of May, twenty-four hours afterwards, I examined a fmall drop of each by the firft magnifier of Wilfon's microfcope, whofe focal diftance is reckoned at $\frac{1}{50}$th part of an inch, and to my amazement they were both full of animalcula of a linear fhape, very diftinguifhable, moving to and fro with great celerity; fo that there appeared to be more particles of animal than vegetable life in each drop.

This experiment I have repeatedly tried, and always found it to fucceed in proportion to the heat of the circumambient air, fo that, even in winter, if the liquors are kept properly warm, at leaft in two or three days the experiment will fucceed.

In Mr. Needham's experiments he calls thefe *fpermatic animals*, Philofophical Tranfactions, vol. XLV. p. 644 and 666; what I have obferved are infinitely fmaller than real fpermatic animals, and of a very different fhape; the truth of which every accurate obferver will foon be convinced of, whofe curiofity may lead him to compare them; and I am perfuaded he will find they are no way a-kin to that furprizing part of nature. And though fome philo-

philoſophers of great reputation have agreed in ſentiment with Mr. Needham, yet I am ſatisfied, that whenever this ſubject is taken up again, and properly attended to, the world will be convinced they have been too haſty in their concluſions.

At preſent I ſhall paſs over many other curious obſervations, which I have made on two years experiments, in order to proceed to the explaining a hint, which I received laſt January from Mr. De Sauſſure, of Geneva, when he was here; which is, that he lately found one kind of theſe *animalia infuſoria*, that increaſes by dividing acroſs into nearly two equal parts.

I had often ſeen this appearance, in various ſpecies, a year or two ago; as I found upon looking over the minutes I had taken when I made any new obſervation; but always ſuppoſed the animals in this dividing ſtate to be in coition.

Not hearing till after Monſieur De Sauſſure had left this kingdom, from what infuſion he had made his obſervation; his friend, Doctor De la Roche, of Geneva, informed me, the latter end of February laſt, that it was from hempſeed.

I immediately procured hempſeed from different ſeedſmen, in diſtant parts of the town: ſome of it I put into New-river water, ſome into diſtilled water, and ſome I put into very hard pump-water; the reſult was, that in proportion to the heat of the weather, or the warmth in which they were kept, there was an appearance of millions of minute animalcula in all the infuſions; and ſome time after, ſome oval ones made their appearance, as at Tab. VI. Fig. 1. *b*. *c*. Theſe were much larger than the firſt, which ſtill continued;

nued; these wriggled to and fro in an undulatory motion, turning themselves round very quick, all the time that they moved forwards. I was very attentive to see these animals divide themselves; and at last I perceived a few of the appearance of Fig. 1. *a*., as it is represented by the first magnifier of Wilson's microscope; but I am so well convinced by experience, that they would separate, that I did not wait to see the operation: however, as the following sketches, which I have drawn from five other species, will very fully explain this extraordinary phænomenon, there will be no difficulty in conceiving the manner of the first. See Fig. 2, 3, 4, 5, and 6.

The proportion of the number of the animals, which I have observed to divide in this manner, to the rest, is scarce 1 to 50: so that it appears rather to arise from hurts received by some few animalcula among the many, than to be the natural manner in which these kind of animals multiply: especially if we consider the infinite number of young ones which are visible to us through the transparent skins of their bodies, and even the young ones that are visible in those young ones, while in the bodies of the old ones.

But nothing more plainly shews them to be zoophytes than this circumstance; that when, by accident, the extremity of their bodies has been shrivelled for want of a supply of fresh water, the applying more fresh water has given motion to the part of the animal that was still alive; by which means this shapeless figure has continued to live and swim to and fro all the time it was supplied with fresh water.

I cannot finish this part of my remarks on these animals, without observing, that the excellent Linnæus

næus has joined the *beroe* with the *volvox*, one of the *animalia infuſoria*. The *beroe* is a marine animal found on our coaſts, of a gelatinous, tranſparent nature, and of an oval or ſpherical form, about half an inch to an inch diameter, divided like a melon into longitudinal ribs, each of which is furniſhed with rows of minute fins, by means of which this animal, like the *animalia infuſoria*, can ſwim in all directions with great ſwiftneſs. In the ſame manner I have ſeen moſt of theſe minute animals, which move ſo ſwift that we could not account for it, without ſuppoſing ſuch a proviſion of nature, which is really true; but cannot be ſeen till the animals grow faint for want of water; then, if we attend, we may, with good glaſſes, plainly diſcover them *.

I come now to a ſingular property, which I have diſcovered in hempſeed, of producing an indiſſoluble

* I have lately found out, by meer accident, a method to make their fins appear very diſtinctly, eſpecially in the larger kind of animalcula, which are common to moſt vegetable infuſions, ſuch as the *Terebrella*: this has a longiſh body, with a cavity or groove, at one end, like a gimblet: by applying them a ſmall ſtalk of the horſe-ſhoe *Geranium* (or *Geranium zonale* of Linnæus), freſh broken, to a drop of water in which theſe animalcula are ſwimming, we ſhall find, that they will become torpid inſtantly, contracting themſelves into an oblong-oval ſhape, with their fins extended like ſo many briſtles all round their bodies; the fins are in length about half the diameter of the middle of their bodies. Before I diſcovered this expedient, I tried to kill them by different kinds of ſalts and ſpirits; but though they were deſtroyed by this means, their fins were ſo contracted, that I could not diſtinguiſh them in the leaſt. After lying in this ſtate of torpidity for two or three minutes, if a drop of clean water is applied to them, they will recover their ſhape, and ſwim about immediately, rendering their fins again inviſible.——For the different ſtates of this animalcule, ſee Tab. VI. Fig. 5. *a*, *b*, *c*, *d*.

salt,

ſalt, when infuſed for ſome time in water: and as hempſeed is known to be an efficacious medicine in ſome particular caſes, theſe experiments may demand a ſtricter enquiry from the profeſſors of phyſic, which may poſſibly turn to the benefit of mankind.

EXPERIMENT I.

On the 25th of February laſt, I put half an ounce of hempſeed to about two ounces of New-river water in a vial, and covered it cloſe with paper, to prevent the duſt coming to it: by the 25th of March it became very putrid, and had thrown up a viſcid ſcum to the top. Fahrenheit's thermometer in the houſe was, during this time, from about 44 to 52 degrees. I examined this ſcum with a common magnifier, of about an inch focus, and could diſcover it to be full of regular-ſhaped ſalts, which lay on the ſurface; ſome of a ſquare, others of an oblong figure.

Applying ſome of the ſcum to a ſlip of glaſs, I placed it in the ſingle or Wilſon's microſcope, making uſe of the fourth magnifier, and it exhibited the cryſtals repreſented before fig. 7; but as the ſtirring of the ſcum had obſcured the preciſe figure of the ſalts, I applied a hair pencil to them, dipt in clean river water, and ſeparated them from the mucilage that had beſmeared them; yet, notwithſtanding this addition of water, their figures were not in the leaſt impaired or melted, but their outlines were rather more exactly defined. Nor were the millions of mi-

nute animals that were ſwimming over them, and all round them, in the leaſt affected by the ſalt *.

I further obſerved, that the cryſtals that appeared firſt increaſed in ſize, and began to vary their forms; for inſtance, many of the cryſtals, at the latter end of April, among the reſt, were of the form of thoſe in the line of fig. 8. About the 5th of May, many of them appeared as at fig. 9; and at the latter end of May, about the 20th, many of them were of the form of thoſe at fig. 10: moſt of the variety of forms appearing at the ſame time.

It was objected by ſome very ingenious men, to whom I had imparted this diſcovery, that theſe ſalts might be owing to ſomething in the water that I had made uſe of, which, joined to the oil in the hempſeed, might produce this appearance. To obviate this:

Experiment II.

I prevailed on my friend, Mr. P. Woulfe, F.R.S. to furniſh me with ſome water that had been moſt carefully diſtilled, by a very ſlow proceſs; and at the ſame time I procured hempſeed from a different part of the town. On the 30th of April, I put an ounce of this hempſeed to about four ounces of this diſtilled water, into a glaſs cylindrical veſſel, and covered it carefully with a glaſs cover; and on the 12th of May I examined the ſcum, and found it more tranſparent,

* Mr. Needham obſerves, in his curious Memoir before mentioned, p. 649, that ſalt deſtroys theſe animalcula; this, I believe, is very true of the common kinds of ſalt; and which renders the nature of this kind of ſalt ſtill more ſingular.

but

but full of the cryſtals of ſalts, as repreſented at fig. 12. Some of the firſt hempſeed put into the ſame water produced much ſalt, but not ſo regular in its figures; theſe figures, by ſome means unknown to me, after their cryſtallization being broke irregularly at their ends, ſee fig. 13. But yet in this infuſion there were many of the original ſeminal figured ſalts.

Experiment III.

I was determined to ſee what effect the hard pump water of Gray's-Inn, after a month's dry weather, would have on the hempſeed in infuſion; particularly as I was perſuaded from experience, that this water contained a large portion of calcareous earth. Accordingly, on the 5th of May, I put an ounce of the ſame hempſeed with the laſt which I had obtained, into four ounces of this pump water; and on the 17th of May I perceived the cryſtals, which, on being put into the microſcope, with the ſame magnifier, gave the appearance repreſented at fig. 14.

The cryſtals of this infuſion ſeemed larger and flatter, and ſomething different in their ſhape; but on examining the mucilage that lay among the ſeeds at the bottom of the glaſs, I found an infinite number of the ſame ſhaped cryſtals with thoſe I have called ſeminal cryſtals; which were likewiſe found in the mucilage of the New-river water infuſion, and in the diſtilled water infuſion among the ſeeds.

I muſt further obſerve, that the calcareous earth floated in great abundance among the ſcum of the pump-water, as ſoon as the putrefaction was ad-

vanced; which did not appear on the ſurface of the diſtilled water, and ſcarcely any on the river water.

The grains of ſalt produced in theſe experiments were about the ſize of the fineſt baſket ſalt, and of a pale yellowiſh colour when dry.

Gray's-Inn,
May 24, 1769.

POSTSCRIPT.

I have ſince found the ſame kind of cryſtals in an infuſion of flax-ſeed in New-river water, and alſo in wheat that has been infuſed in boiling hot water; but the cryſtals were fewer, and did not appear ſo ſoon in the flax-ſeed as in the hemp-ſeed. And the experiment of wheat infuſed in boiling hot water does not always ſucceed.

I have likewiſe found ſalts not unlike thoſe of the hempſeed, in infuſions of a variety of pulſe and grain from the Eaſt Indies, ſuch as lupines, kidney-beans, vetches, millet, Guinea-corn, and the ſeſamum or oily grain: but the laſt yielded a much larger quantity of ſalt, and in a ſhorter time, than any of the reſt.

The ſalts of theſe different ſubſtances were alſo not diſſolvable upon applying clean water to them; but by letting the infuſions continue to putrefy ſome weeks longer, they by degrees aſſumed irregular ſhapes, and diſappeared. I muſt conclude, then, with this quere, Are not theſe the oily parts of the vegetables, which float in the ſcum, on the ſurface of the infuſion, cryſtallized?

Explana-

Explanation of the FIGURES, TAB. VI.

Thefe five different kinds of *animalia infuforia*, belonging to the genus of *Volvox* of Linnæus, are here reprefented both in their perfect and in their divided ftate. The trivial names are added to diftinguifh the fpecies.

Fig. 1. reprefents the *volvox ovalis*, or egg-fhaped volvox: at (*c*) and (*b*) it is expreffed in its natural fhape: at (*a*) the manner in which it becomes two animals, by feparating acrofs the middle: this was found in the infufion of hempfeed, but is found in other vegetable infufions, particularly in that of tea-feed.

Fig. 2. is the *volvox torquilla*, or wryneck. At (*a*) is reprefented its divided ftate, at (*b*) and (*c*) its natural fhape; this is common to moft vegetable infufions, as is the following.

Fig. 3. is the *volvox volutans*, or the roller. At (*a*) the animal is feparated, and becomes two diftinct beings, each fwimming about and providing for itfelf; this is often the prey of another fpecies of this genus, efpecially while it is weak by this feparation, not being fo active for fome time till it can recover itfelf. At (*c*) the animal appears to be hurt on one fide; this impreffion, in a little time, is fucceeded by another on the oppofite fide, as at (*b*), which foon occafions a divifion.

At

At (*d*) is the ſide view, and at (*e*) the front view of the natural ſhape of the animal.

Fig. 4. is the *volvox oniſcus*, or wood-louſe. At (*a*) is the natural ſhape of it, as it appears full of little hairs both at the head and tail; with thoſe at the head it whirls the water about, to draw its prey to it; the feet, which are many, are very viſible, but remarkably ſo in a ſide view at (*d*). At (*b*) it is repreſented beginning to divide, and at (*c*) the animals are ready to part: in this ſtate, as if in exquiſite pain, they ſwim round and round, and to and fro, with uncommon velocity, violently agitated till they get aſunder. This was found in an infuſion of different kinds of pine branches.

Fig. 5. is the *volvox terebrella*, or the gimblet. This animal is one of the largeſt of the kind, and is very viſible to the naked eye. It moves along ſwiftly, turning itſelf round as it ſwims, juſt as if boring its way. (*a*) and (*b*) are two views of its natural ſhape. (*c*) ſhews the manner of its dividing. When they are ſeparated, the lower animal rolls very aukwardly along till it gets a groove in in the upper part. (*d*) repreſents one of them lying torpid, by means of the juice of the *horſeſhoe geranium*, with its fins extended. This animal is found in many infuſions, particularly of graſs or corn.

Fig. 6. is the *volvox vorax*, or the glutton. This animal was found in an infuſion of the Tartarian pine; it varies its ſhape very much,

contracting

contracting and extending its probofcis, turning it to and fro, in various directions, as at *a*, *b*, *c*, *d*, *e*. It opens its probofcis underneath the extremity, when it feizes its prey. The lefs active animals, that have lately been divided, fuch as thofe at Fig. 2. *a*. and Fig. 3. *a*, ferve it as food when they come in its way: thefe it fwallows down inftantly, as it is reprefented at Fig. 6. *h*. and *i*. At (*f*) it is ready to divide, and at (*g*) it is divided, where the hinder part of the divided animal has got a probofcis or beak, to procure nourifhment for itfelf, and foon becomes a diftinct being from the fore-part.

Fig. 7. reprefents the appearance of the falts in hempfeed, after a month's infufion, from the 25th of February to the 25th of March, in New-river water.

Fig. 8. The falts, about a month after, April 25, appeared in this manner.

Fig. 9. Thefe figures reprefent them about the 5th of May, or ten days after.

Fig. 10. About the 20th of May, they exhibited the figure of precious ftones.

Fig. 11. Thefe I have called feminal falts, as thefe fmall figures are to be feen in moft of the infufions, rifing at different times, and exhibiting thefe fhapes, when they firft appear diftinctly,

Fig. 12. reprefents the falts of hempfeed in diftilled water, that had been infufed from the 30th of April to the 12th of May.

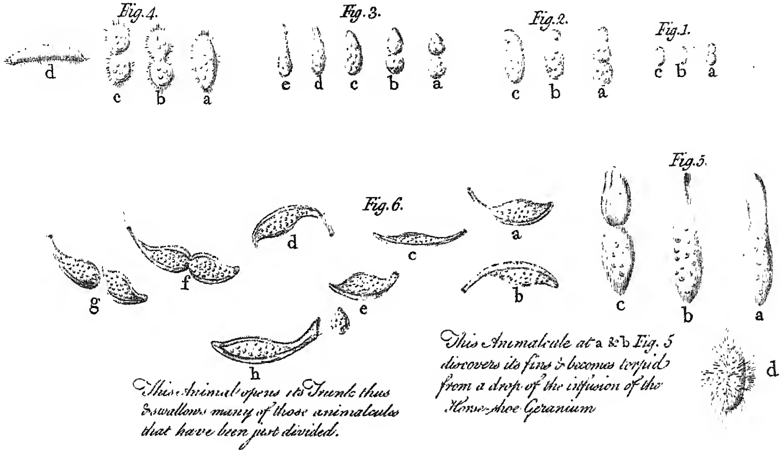
Fig.

Fig. 13. ſhews the form of the ſalts when the putrefaction had begun to ſeparate their parts into laminæ in the diſtilled water.

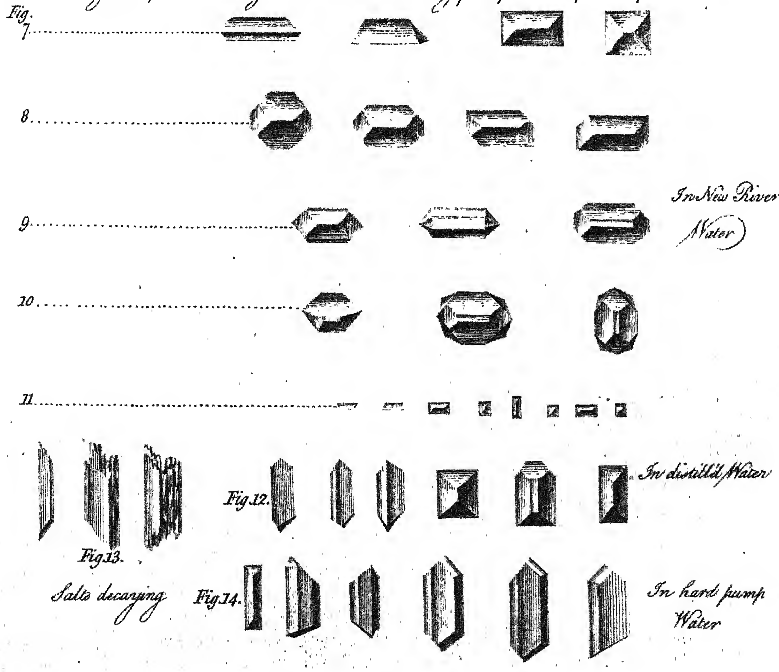
Fig. 14. are the figures of the ſalts that appeared from the hempſeed, infuſed in hard pump-water about twelve days, from the 5th of May to the 17th.

XIX. *On*

The Figures of 6 kinds of Animalcula infusoria, that encrease by dividing across the middle into two distinct animals.



Figures of Salts, not dissolvable in Water, arising from a putrid infusion of Hempseed.



XIX. *On the Computation of the Sun's Distance from the Earth, by the Theory of Gravity: In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev. Mr.* Horsley, *F. R. S.*

SIR,

Read June 1, 1769.

A LITTLE Treatise, that has lately been published, against Dr. Stewart's method of determining the distance of the Sun by the theory of gravity, has put me upon reconsidering a subject which I had long dismissed from my thoughts. I am far from being convinced that Dr. Stewart's conclusions are "erroneous upon "his own principles," as his antagonist affirms; and I am well satisfied that there is no error in the principles themselves. I have always been sensible that an extreme precision was requisite in determining the mean quantity of the solar force affecting the moon's gravity towards the earth, in order to obtain an accurate estimation of the distance; and this circumstance was mentioned by me, in a paper that I communicated to the Society about two years ago *, before it had been remarked, that I knew of, by any other writer upon the subject. I must now declare, that the imperfection of the method arising from this

* See Philos. Trans. vol. LVII. for 1767, p. 179. 183.

circumſtance is much greater than I was at firſt aware of. I owe this better information entirely to the reviſal of Dr. Stewart's Theorems, not to any thing that has been written upon the ſubject by others. I find that if I increaſe the mean quantity of the ſun's diſturbing force, as determined by Dr. Stewart in the 9th propoſition of his Supplemental Tract, and by myſelf, in my former paper, by $\frac{1}{25000}$th part of itſelf, I obtain, by my own method of computation, 9″ 3′′, 394 for the Sun's mean horizontal parallax; which ſeems to be ſo nearly the mean of the quantities of the parallax deduced from the beſt obſervations of the tranſit of 1761, that it would be ridiculous to ſet up, any longer, the concluſions of this theory in oppoſition to obſervation. It is much more probable that the theory ſhould err in ſo ſmall a matter as $\frac{1}{25000}$th of the Sun's diſturbing force, than that obſervation ſhould err in more than $\frac{2}{9}$, that is nearly in $\frac{1}{4}$ of the whole quantity in queſtion. I beg the favour of you to communicate this to the Society.

I have the honour to be, Sir,

Your moſt obedient,

and moſt humble ſervant,

Oxford, May 5,
1769.

Samuel Horſley.

XX. *Meteora-*

XX. *Meteorological Obſervations for* 1768, *made at* Bridgwater *in* Somerſetſhire, *and at* Ludgvan *in* Mount's-Bay, Cornwall. *Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.*

Read June 1, 1769.

OBSERVATIONS at BRIDGWATER, 1768.

1768. Month.	Barometer.			State of the Weather and Wind.	Fahrenheit's Thermom.			Ombr.
			In.				Med.	In.
January	3. 24.	Higheſt Loweſt	30,14 29,18	From the 1ſt to the 8th, froſty and remarkably cold. On the 9th a very deep ſnow, 1 foot 3 inches undriven; and it would have been deeper, had it not rained the day before. From thence, to the end of the month, cloudy, foggy, or rainy.—The wind northerly the firſt part, and in the latter variable from thence to N. E.	25. 4.	Higheſt 48 Loweſt 9	34	3,905
February	5. 10.	Higheſt Loweſt	30,42 29,27	Changeable throughout; either cloudy or rainy, with very little ſun-ſhine. The wind either N. E. or N. W. the firſt part of the month, and generally S. W. the remainder. Very high in the middle of the month.	11. 3.	Higheſt 53 Loweſt 28	$43\frac{15}{29}$	2,390
March	4. 15.	Higheſt Loweſt	30 46 29,73	The firſt part generally cloudy, the middle ſun-ſhine, and the latter part cloudy. The wind either N. E. or N. W. almoſt the whole month, and remarkably calm, except two or three days in the middle.	29. 4.	Higheſt 55 Loweſt 28	$40\frac{22}{31}$	,125
April	1. 29.	Higheſt Loweſt	30,21 29,30	Conſtant ſun-ſhine for the firſt five days, but from thence to the end of the month generally ſtormy. The wind chiefly weſterly the whole month.	14. 3.	Higheſt 59 Loweſt 39	49	2,829

 May

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.		Ombr.
	In.			Med.	In.
May	15. Higheft 30,22 29. Loweft 29,24	Stormy for a few days in the beginning, but generally fun fhine through the remainder. The wind chiefly northerly. N. B. A very remarkably ftrong Aurora Borealis on the 4th, and very uncommon, almoft conftant, lightning for feveral hours, on the 5th, but without injury.	23. Higheft 74 1. Loweft 40	55	1,270
June	2. Higheft 30,15 9. Loweft 29,26	Sun-fhine for the firft feven days: from thence ftormy. cloudy, and fun-fhine, intermixed. The wind variable, between N. W. and S. W.	6. Higheft 69 19. Loweft 48	$57\frac{15}{30}$	3,935
July	21. Higheft 30,05 7. Loweft 29,43	Generally rainy, and chiefly towards the latter end. The wind generally S. W.	28. Higheft 73 9. Loweft 50	61	4,565
Auguft	8. Higheft 30,12 23. Loweft 29,43	Changeable throughout. Sun-fhine, cloudy, and rain. The wind moft commonly northerly.	14. Higheft 69 29. Loweft 52	$61\frac{10}{31}$	2,485
September	14. Higheft 30,12 16. Loweft 28,94	The firft and laft weeks cloudy and fun-fhine intermixed. But almoft conftant rain during the middle of the month. The wind generally S. W.	27. Higheft 65 14. Loweft 46	$55\frac{1}{30}$	6,207
October	19. Higheft 30,14 5. Loweft 29,14	The firft part generally cloudy, with hard ftorms. The middle cloudy and fun-fhine intermixed; and the latter part ftormy. The wind chiefly wefterly.	7. Higheft 63 22. Loweft 40	$50\frac{23}{31}$	4,015
November	6. Higheft 30,33 22. Loweft 28,23	Changeable throughout. The wind generally either N. W. or S. W. and high. The barometer was remarkably low the latter part of this month.	8. Higheft 59 9. Loweft 36	$42\frac{8}{30}$	5,595
December	12. Higheft 30,43 2. Loweft 28,89	The firft part changeable; fmart froft in the middle, and generally cloudy in the latter part. The wind generally S. W.	8. Higheft 56 15. Loweft 22	$41\frac{20}{31}$	2,170

Total of Rain 39,291

OBSER-

OBSERVATIONS at LUDGVAN in MOUNT'S-BAY, CORNWALL, 1768.

By WILLIAM BORLASE, D. D.

1768. Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
			Med.	In.
January	3. Highest 29,99 1. Lowest 28,88	On the 1st stormy, thunder, lightning. Hard frost, with some snow, from the 2d to the 6th. The 6th a. m. snow gone, but the ice in a garden watering-pot found ten inches deep. The 7th thaw with much rain. 8th a. m. frost all gone. On the 9th melting snow and rain. No more snow or frost. Rest mostly stormy with rain. Wind chiefly East and North.	25. Highest 49 3. Lowest 25 } $40\frac{15}{31}$	6,900
February	6. Highest 30,29 10. Lowest 29,10	Cloudy. 10th a violent storm, wind S. by W.; extreme at noon. The rest misty, stormy, showery, and rain. Little sun-shine or calm. Wind blew twenty days from the West.	21. Highest 54 3. Lowest $34\frac{1}{2}$ } $47\frac{1}{29}$	3,760
March	11. Highest 30,27 5. Lowest 29,69	Gentle frosts, for a day only, on the 4th, 8th, and 24th. Rest mostly fair and sun-shine. Barometer unusually high. Wind chiefly from the East and North.	17. Highest 55 4. Lowest 33 } $44\frac{27}{31}\frac{1}{2}$	0,350
April	1. Highest 30,3 29. Lowest 29,10	Weather windy, with frequent showers. The wind in the first third of the month East, and East-North-East, rest Westerly, mixed with the South.	18. Highest 56 1. Lowest 41 } $49\frac{26}{30}$	3,180

May

Month.	Barometer.			State of the Weather and Wind.	Fahrenheit's Thermom.			Med.	Ombr.
			In.					Med.	In.
May	23. 29.	Highest Lowest	30 5 29,10	Much calms and sun-shine. Wind mostly from the East, mixed with the North. Pleasant month.	25. 2.	Highest Lowest	70 43	$54\frac{16}{31}\frac{1}{2}$	1,550
June	2. 11.	Highest Lowest	30,4 29,10	On the 6th thunder, lightning, and rain. The 25th ditto. On the 11th stormy. 27th ditto. Rest fair, sun-shine and rain intermixed. Wind equally from all points.	3. 13.	Highest Lowest	$66\frac{1}{2}$ 49	$56\frac{12}{30}$	4,750
July	21. 7.	Highest Lowest	29,90 29,18	Showery, rainy, and fair, intermixed. Windy and calm nearly equal. Stormy rain on the 26th. Wind blew twenty-six days from the West, mostly mixed with a point of the South.	30. 17.	Highest Lowest	66 53	$59\frac{5}{31}$	4,900
August	8. 22.	Highest Lowest	30,3 29,31	To the 16th fair, generally, with sun-shine, and much calms. On the 17th p. m. thunder, lightning, and violent rain, of which at least one inch fell from 2 p. m. to 5 p. m. N. B. Great falls of rain on the same day in Yorkshire, Wales, &c. Wind then at East and North. Rest mostly from the West.	16. 2.	Highest Lowest	$67\frac{1}{2}$ 55	$60\frac{5}{31}$	3,900
September	14. 16.	Highest Lowest	30,6 28,97	Beginning showery, calm, and sun-shine. On the 12th violent rain and wind. 13th, 22d, ditto. On the 13th, 17th, 23d, 25th, 26th, stormy. On the 16th a storm. Wind W. $\frac{1}{2}$ S. One half of the month wind from the Westward; the rest mixed.	28. 21.	Highest Lowest	64 47	$55\frac{24}{30}\frac{3}{4}$	4,510
October	19. 5.	Highest Lowest	30,2 28,99	Rainy, windy month. On the 5th and 6th stormy rain. 7th, 18th, 29th, ditto. On the 6th thunder, lightning, and hail. One half of the month wind blew from the West; rest mixed.	1. 13.	Highest Lowest	61 45	$52\frac{23}{31}\frac{1}{4}$	5,010

November

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
November	In. 6. Higheſt 30,17 22. Loweſt 28,23	On the 4th violent hail-ſhowers. On the 2d hard rain. 10th, 11th, 21ſt, 26th, 27th, 28th, 29th, ditto. On the 4th, 16th, 21ſt, 22d, 23d, 24th, 28th, 29th, ſtormy. Wind blew twenty-ſeven days from the Weſt. N. B. From Sunday the 20th $8\frac{3}{4}$ p. m. to Tueſday 22 half hour p. m. (viz. in $39\frac{3}{4}$ hours) the mercury in the barometer fell 175 (all decimals), and at half paſt noon was down to 28,23, the loweſt yet noted here. Wind W. $\frac{1}{2}$ N. By $8\frac{1}{2}$ p. m. of the 22d, the mercury was riſen to 28,31.	Med. 1. Higheſt 54 22. Loweſt 41 } $48\frac{15}{30}\frac{1}{4}$	In. 6,930
December	17. Higheſt 30,17 2. Loweſt 28,70	Violent rains. From December 1, 10 a. m. to December 2 noon, in 26 hours the fall of rain was 1 inch $\frac{250}{1000}$th. On the 6th lightning, and ſtormy ſhowers. On the 16th a ſtorm, wind S. E. On the 13th ſmart froſt. 14th ditto. 15th a deep fall of ſnow; but that, and the froſt, diſmiſſed, by the next morning, by ſtormy, violent rains, about 1 inch having fallen on the 15th at night. Wind moſtly from the South and Weſt.	21. Higheſt 52 12. Loweſt 40 } $46\frac{8}{31}$	5,150

Rain fallen in the Year 1768 50,890

XXI. *Propoſal*

XXI. *Proposal of a Method for securing the Cathedral of* St. Paul's *from Damage by Lightning; in consequence of a Letter from the Dean and Chapter of* St. Paul's *to* James West, *Esquire, Pr. R. S.*

March 9, 1769.

The following Letter to the President was read; videlicet,

St. Paul's, March 6, 1769.

SIR,

THE consideration of the old church of St. Paul's having twice suffered by lightning, and a solicitude to secure the present fabric from similar accidents; which, but for the interception of the storm by St. Bride's church, within these few years, might have already happened; induce us, the Dean and Chapter of this cathedral, to request the opinion of the Royal Society (so justly eminent for the abilities of its members in every branch of science), relative to the best and most effectual method of fixing electrical conductors. We shall esteem ourselves obliged to the very respectable body over which you preside,

presside, for their sentiments and directions on this subject, and are, with much regard,

S I R,

Your most obedient,

humble servants,

Thomas Bristol, D.
Chr. Wilson.
S. Barrington.
J. Lich. & Cov.

To James West, Esquire, President of the Royal Society.

In consequence of this application, it was desired that John Canton, M. A. Edward Delaval, Esquire, Benjamin Franklin, LL.D. William Watson, M. D. and Mr. Benjamin Wilson, be a Committee to consider the above letter, and report their opinion thereon to the Society; and, accordingly, June 8, 1769, Dr. Watson, at the meeting of the Society, read, in his place, a report from the Committee appointed to consider the application from the Dean and Chapter of St. Paul's, relating to the preservation of that elegant structure from damage by lightning; for which report, thanks were ordered to the Committee, and returned to Dr. Watson: and it was also ordered, that a copy of the said report be transmitted to the Dean and Chapter of St. Paul's, signed by the Secretary.

Report from the Committee appointed to consider of the properest means to secure the Cathedral of St. Paul's *from the Effects of Lightning. Addressed to* James West, *Esquire, President of the Royal Society.*

SIR,

Read June 8, 1769.

AS, in consequence of a letter addressed to the Royal Society from the Dean and Chapter of St. Paul's, the Society did us the honour to appoint us a Committee to examine that magnificent structure, and, as far as our experience would enable us, to prevent mischief thereto from lightning, by a properly disposed apparatus; we lay before you the following as our opinion thereupon, to be communicated, if you think proper, to the Royal Society. And here, Sir, you will permit us to take notice of, and acknowledge, the obligations we were under to Mr. Mylne, a very worthy member of this Society, who is surveyor of St. Paul's, and attended several meetings of the Committee. This gentleman furnished us with a great variety of information, in relation to the structure of the several parts of this fabric, which, without his assistance, could not easily have been obtained.

As all metals are now known readily to conduct or transmit the electric fluid, or, which is the same thing, lightning, through them; the large quantity of lead, and some iron, disposed in different parts of St. Paul's church, will, by having its several parts connected,

connected, where there is at present no such connection, prevent the erecting a considerable part of the apparatus, which otherwise we should judge absolutely necessary.

We are of opinion that, *cæteris paribus*, all buildings upon the same level are liable to be injured by lightning in proportion to their height: and that the danger is increased by crosses, weather-cocks, or pieces of metal, in any form, placed upon or near their tops, unless there is a compleat metallic communication from these to the bottom of the building, which metal should terminate either in water, or moist ground.

In St. Paul's church, the objects of our more particular attention were the dome and its lanthern, and the two towers at the west end. The roof over the body of the church, being compleatly covered with lead, will, we conceive, prevent mischief thereto from lightning; and the more so, as the lead on the roof joins to that of the several leaden spouts, which come down the sides of the building, and terminate in the ground at a considerable depth. For our more certain information, one of these spouts was examined; and it was found to descend perpendicularly about three feet under the surface of the earth: and then, after being laid about seven feet in an inclined direction, it ended in a brick drain, which communicates with the sewer. These circumstances induce us to conclude, that what has been just now described is a sufficient metallic communication between the roof of the church and the ground.

No part of this whole fabric seems to be in so dangerous a situation of being injured by lightning, as the

the ſtone lanthern placed above the dome. This danger ariſes not only from its height, but from the different pieces of metal in different parts of it, being at preſent detached and ſeparated from each other. This ſtone lanthern is ſupported by a truncated cone of brick-work, of no more than eighteen inches, or two bricks, thick. To the honour, however, of the architectural ſagacity of Sir Chriſtopher Wren, who was formerly our Preſident, this ſupport of the lanthern, which has already ſtood much above half a century, has not in the leaſt given way in any of its parts. How far it would ſuſtain the violence of a ſtroke of lightning will, it is to be hoped, never be tried: and what we have now to propoſe will, we flatter ourſelves, leſſen the probability of its being Injured by it. The firſt object of our attention, therefore, was to make a compleat metallic communication between the croſs, placed over this lanthern, and the leaden covering of the great dome; as from its height, if any lightning was in its neighbourhood, it would moſt probably affect the croſs.

This croſs with the ball, both compoſed of metal, are ſupported by, and connected with, ſeven iron rods. Theſe deſcend perpendicularly through the ſmall leaden dome, which covers the lanthern, and are inſerted into and paſs through a ſtrong frame of timber, placed horizontally under that dome. The lower extremities of theſe iron rods are faſtened to the under ſurface of this timber frame with iron nuts and ſcrews.

From this timber work, ſeveral large iron bars, placed at ſome diſtance from the ends of the abovementioned iron rods, deſcend obliquely, and are fixed

in

in the ſtone-work of the lanthern. The upper ends of each of theſe oblique iron bars paſs through the frame of timber before mentioned, and are faſtened to its upper ſurface with iron nuts and ſcrews. Between theſe iron bars and the leaden covering of the great dome, there is at preſent no metallic communication. To this arrangement, therefore, is owing the danger from lightning, which the Committee apprehends that this part of the building is liable to. To obviate which, we are of opinion, that four additional iron bars, each not leſs than an inch ſquare, ſhould be ſecurely placed over the frame of timber before mentioned in ſuch a manner, that one end of each of theſe four additional iron bars may be in contact with one of the perpendicular iron rods, and the other end of each be in contact with one of the iron nuts and ſcrews, which faſten the obliquely deſcending iron bars to this frame of timber. At the bottom of theſe oblique iron bars, juſt above where they are inſerted into the ſtone-work, the Committee recommends, that a ring, made of bar iron, of about an inch ſquare, ſhould be placed ſo as to be faſtened to, and be in contact with, theſe iron bars.

From this propoſed ring to the upper part of the lead which covers the great cupola, the diſtance is about forty-eight feet. In this ſpace, we are of opinion, that four iron bars ſhould be placed, each not leſs than an inch ſquare. Theſe ſhould be fixed within the lanthern in ſuch a manner, that the upper end of each ſhould be faſtened to, and in contact with, the iron ring before mentioned, and their lower ends in contact with the lead on the upper part of the cupola; from which the metallic communication is

compleat

compleat to the lower end of the pipes, that difcharge the water from the circular part of the great cupola, upon the floor of the ftone gallery.

From the bottoms of thefe pipes, which terminate with a fhoe of lead within half a foot of the floor of the ftone gallery, the metallic communication is again interrupted to the top of the leaden pipes, which convey the water from thence. Here it is propofed, that conductors of lead, not lefs than four inches in breadth and half an inch in thicknefs, fhould be placed fo as to be in contact with the bottom of four of the pipes that come from above, and with the top of four of thofe that defcend. Lead is recommended to be employed here, as more readily adapting itfelf to the various curvatures it muft meet with in the now propofed arrangement.

Thefe laft pipes, after defcending below the colonnade, near the circular ftair-cafes, make their appearance upon the outfide of the drum-part of the cupola; where they are bent at obtufe angles, and difcharge their water upon the roof of the church. From thefe angles to the roof the diftance is about five feet. Here then is another interruption to the metallic communication. This is propofed to be compleated by conductors of lead, fimilar to thofe before mentioned, which fhould be fo placed as to be in contact both with the bottom of the pipes and the adjoining roof.

From the roof, as has already been mentioned, the leaden pipes are continued below the furface of the earth, and terminate in a drain; and thus, by the method now directed, the metallic communication will be compleated from the crofs on the top of

St. Paul's church to ſome feet below the ſurface of the ground.

The Committee then turned their thoughts towards the two towers at the weſt end of the church; and here they beg leave to obſerve, that in one of theſe towers, between the pine apple and the leaden bell-ſhaped covering near it, placed at the top of each of theſe towers, there is no metallic communicatson deſerving notice, till you come to the lead on the roof of the church. This diſtance is eighty-eight feet. To this tower, therefore, it is propoſed to adapt a rod or bar of iron, not leſs than an inch and a quarter ſquare, in ſuch a manner that one end of the bar ſhould be in contact with the metal communicating with the pine apple on its top, which is of copper, and the other end with the lead on the roof of the church.

In the middle of the other tower, in which the great bell is hung, there is an iron ſtair-caſe of conſiderable height, which is placed in the middle of it, in order for the more conveniently coming at the clock-work. The top of this ſtair-caſe is at no great diſtance from the leaden covering upon the top of the tower: but from the bottom of this ſtair-caſe to the roof of the church, between which there is no metallic communication, the diſtance is conſiderable, not leſs than forty feet. The Committee recommend, therefore, that a bar of iron, of an inch and a quarter ſquare, may be placed between the pine-apple; or the lead in contact with it, and the upper part of this ſtair-caſe; and that another iron bar, ſimilar to this laſt, may be adjuſted ſo, as to paſs from the bottom of the ſtair-caſe to the lead on the roof of the church. The roof,

as

as has been already mentioned, communicates with the leaden pipes, and theſe with the ground.

Theſe towers, from their near ſituation to the cupola, which is a building ſo much higher, may poſſibly be leſs liable to miſchiefs from lightning than if they were erected at a more conſiderable diſtance. As the direction of the lightning is, however, uncertain, from a variety of cauſes, as alſo to what extent one building will protect another, the Committee are of opinion, that this apparatus to the towers will be expedient.

It is to be remarked, that wherever iron is employed as a conductor of lightning, eſpecial care muſt be taken to prevent its becoming ruſty; as, from being long expoſed to the moiſt atmoſphere, it will be corroded to a conſiderable depth: and ſo much of the iron as is corroded ceaſes to be of uſe as a conductor; the Committee therefore have, in directing the ſize of theſe iron bars, made ſome allowance for the waſte of the iron by ruſt.

The ſize, as well as number, of the iron bars recommended here by the Committee, are only to be conſidered as applicable to St. Paul's, and not as a ſtandard for any church or building of leſs dimenſions; as in theſe laſt, conductors of a ſmaller ſize, and fewer in number, may anſwer the purpoſe as ſecurely as the larger. But St. Paul's church is particularly circumſtanced: it is an edifice not only of great height, but its cupola, to ſay nothing of the lead on the body of the church, preſents a large ſurface of metal to the clouds; on which account it is very liable to receive greater quantities of the electric fluid; and, from large quantities of ſuch an elaſtic power,

great

great mifchiefs may arife to this magnificent building, in confequence of obftructions the fluid may meet with in paffing through it. For thefe reafons we have recommended very large conductors, that it may pafs through them into the ground, as readily as it enters.

Thefe, Sir, are our fentiments in relation to the matter, referred to us by the Royal Society, upon the requeft of the Dean and Chapter of St. Paul's. If they fhould be acceptable to the Society, and by their means to the Dean and Chapter; and if, by being carried into execution, they fhould at all contribute to the prefervation of that noble fabric, it will be a great fatisfaction to us. We are, with very great refpect,

SIR,

Your moft obedient,

humble fervants,

W. Watfon.
B. Franklin.
B. Wilfon.
John Canton.
Edward Delaval.

7 June, 1769.

XXII. *Observation of the late Transit of* Venus: *In a Letter to* James Burrow, *Esquire, V. P. R. S. By Mr.* James Horsfall, *F. R S.*

SIR,

Read June 8, 1769.

LOOKING upon it as the duty of every member of the Royal Society, to contribute their mite towards the discovery of such an important matter as *The dimensions of the solar system*, I therefore take the liberty to communicate to you the following observations, made upon the late transit of Venus.

They were made with a Gregorian telescope, magnifying at least 100 times.

The time undermentioned is equal time.

The rate of going of the two clocks had been correctly ascertained by my friend Dr. Bevis, and myself, by observing the Sun pass the meridian for several days preceding the day of the transit.

My situation was upon a platform laid upon the ridges of my own chambers near the Middle Temple Hall: consequently, the great volumes of smoke arising from the houses to the north-west were no small impediment to a good observation: this was very remarkable at the *time of internal contact*. The Sun's limb undulated then prodigiously, and there was also a gust of wind which made the telescope vibrate, but

not

not ſo much as once to loſe the planet out of the field. I pronounced that moment (I mean that of internal contact) as ſoon as I ſaw a lambent light (*not* a *well-defined* light) whirl round the opaque limb of the Planet; from whence I am inclined to think I pronounced that *too early* by two or three ſeconds.

The firſt part of this rare phænomenon, which I beheld, was a kind of penumbra, at $7^h\ 8'\ 50''$.

I then counted 1, 2, 3 to $8''$, and plainly diſcerned the dark limb of Venus make a dent very near the vertex of the Sun's limb.

At $7^h\ 26'\ 34''$ I perceived the lambent light abovementioned; at which time the Sun was not above $\frac{1}{4}$ of a degree above the top of a chimney.

This is a ſimple and faithful relation of what I obſerved, which I had not even copied till this morning, after you was pleaſed to notify your expectation.

I am glad of this opportunity publicly to acknowledge the many obligations you have been pleaſed to confer on me; and am,

SIR,

(with great reſpect),

Your much obliged,

and moſt obedient,

humble ſervant,

J. Horsfall.

Middle-Temple,
8 June, 1769.

XXIII. *An*

XXIII. *An Account of the Observations of the Transit of* Venus *and of the Eclipse of the* Sun, *made at* Shirburn Castle *and at* Oxford. *By the Reverend* Thomas Hornsby, *M. A. F. R. S. and* Savilian *Professor of Astronomy in the University of* Oxford.

Read June 15, 1769.

THE weather, on the morning of the 3d of June, was so very unfavourable, both at the observatory of the Earl of Macclesfield and also here at Oxford, that there was very little reason to expect that we should be able to make any observation. But here, a few minutes before noon, the clouds began to break, and I was enabled to observe the transit of the Sun's consequent limb over the meridian. At one o'clock in the afternoon, the sky was again overcast, and it rained for some time; but towards three o'clock, the clouds were dispersed, the Sun shone out clearly, and at five o'clock there was hardly a cloud to be seen. The preceding evening was also so very favourable, that the several persons who proposed to make observations of the transit, had an opportunity of adjusting their instruments.

The Right Honourable the Earl of Macclesfield made use of an excellent refracting telescope of $3\frac{1}{2}$ feet,

feet, made by Mr. Dollond, with a treble object-glaſs, magnifying 150 times; and at $7^h\ 7'\ 49''\frac{1}{2}$ apparent time, was certain that the planet had ſenſibly advanced upon the Sun's diſk, having ſeen a ſmall impreſſion upon the zenith part of the Sun's limb near a minute ſooner. At $7^h\ 23'\ 13''$ mean time, or $7^h\ 25'\ 28''\frac{3}{4}$ apparent time (as reduced from ſidereal time), his Lordſhip determined the internal contact, which he judged to happen when the dark penumbra, which was ſo ſenſibly perceived between the limbs of the Sun and Planet, was loſt upon the completion of the thread of light. His Lordſhip obſerved at a ſmall diſtance from the obſervatory, by means of a ſtop-watch, which was let go at the inſtant he judged the total ingreſs to happen, and immediately compared with the obſervatory clock.

Mr. Bartlett, a very excellent obſerver, who has been conſtantly employed in the obſervatory for many years, obſerved with a 14 feet refractor on the north ſide of the obſervatory, within hearing of the clock, the ſeconds of which were counted by Mr. Phelps, the other aſſiſtant obſerver. At $7^h\ 7'\ 4''$ apparent time, Mr. Bartlett firſt ſaw Venus upon the Sun; and at $7^h\ 23'\ 10''\frac{1}{2}$ mean time, or $7^h\ 25'\ 26''$ apparent time, he judged the ingreſs to happen, the teleſcope magnifying near 60 times.

Lady Macclesfield was alſo pleaſed to attend to the obſervation; and at $7^h\ 25'\ 16''\frac{1}{2}$ apparent time, judged the ſecond internal contact to happen, with a refracting teleſcope of 6 feet, through which the penumbra before mentioned was hardly to be diſtinguiſhed.

The ſky, though free from clouds, was charged with vapour, which occaſioned a conſtant undulation of the limbs of the Sun and Planet; and the wind ſometimes blew ſo hard as to incommode the obſervers.

On the next morning the ſky was very favourable to obſervation, and Mr. Phelps determined the eclipſe of the Sun to begin at $18^h\ 32'\ 45'',7$ mean time, or $18^h\ 34'\ 56'',7$ apparent time, and to end at $20^h\ 17'\ 23'',5$ mean time, or $20^h\ 19'\ 33'',8$ apparent time. The Earl of Macclesfield obſerved the end to happen one ſecond later, making uſe of Mr. Dollond's refractor.

The latitude of the obſervatory at Shirburn Caſtle is $51°\ 39'\ 22''$, as determined by obſervations of the Pole Star, at ſeveral different times; and is $3'\ 57''$ of time weſt of Greenwich, and $1'\ 6''$ to the eaſt of Oxford, as appears by computing the difference of meridians between Mr. Short's houſe, Shirburn Caſtle, and Oxford, as they reſult from the obſervations of the Sun's eclipſe on April 1, 1764.

I propoſed to obſerve the tranſit of Venus and the Sun's eclipſe in the upper room of the tower of the Schools, which, though the floor of it be very unſteady, yet from its elevated ſituation afforded me the cleareſt view of the north-weſt part of the horizon, and is indeed the beſt place for making occaſional obſervations in different parts of the heavens, and at different altitudes, which this place at preſent affords. The clock, furniſhed with a compound pendulum, was for ſome time carefully compared with another clock

clock of the ſame conſtruction, which is fixed in a ſmall obſervatory in the houſe where I live, and which I had altered from ſidereal to mean ſolar time, for the eaſier compariſon of thoſe clocks, which ſeveral gentlemen had procured, in order to obſerve this rare and curious phænomenon. The time was determined by meridional tranſits of the Sun, taken with a tranſit inſtrument made by Mr. Bird, and placed very exactly in the plane of the meridian, the focal length of the object-glaſs being 43 inches. The motion of both clocks was perfectly even and regular.

The atmoſphere was ſo loaded with vapour, and the limb of the Sun was in ſuch a conſtant ſtate of undulation, that I determined to obſerve the external contact with a refractor of 12 feet, furniſhed with a ſyſtem of eye-glaſſes, and magnifying 68 times. I had found, by a previous computation, that the Planet would make the firſt impreſſion upon the Sun's upper limb, about nineteen minutes of a degree to the right hand of a vertical circle paſſing through the Sun's center. I therefore kept my eye conſtantly fixed upon that part, and at $7^h\ 5'\ 58''$ apparent time, I perceived that a ſmall part of the Planet's diameter had certainly entered upon the Sun's diſk; the impreſſion, which I had obſerved for a few ſeconds before, having continued upon that part. While the Planet was paſſing over the Sun's edge, I determined, with the old micrometer applied to the 12 feet glaſs, the following differences of declination between the northern limb of the Sun, and the ſouthern limb of Venus, with as much accuracy as the unſteadineſs of the floor would permit.

Mean

	Mean Time.				
	h	′	″		
At	7	8	48	3	35,2
	7	11	37	3	44,5
	7	13	28	3	46,4
	7	14	57	3	50,1
	7	15	42	3	53,6

But as the time of the internal contact began to draw nigh, I directed a refractor of $7\frac{1}{2}$ feet, with a double object-glaſs, to the Sun, made by Mr. Dollond, and magnifying 90 times; and ſoon after $7^h\ 21'$ mean time, perceived that the Planet appeared to be wholly entered upon the Sun, though the limbs of the Sun and Venus were not actually ſeparated; that part of the Sun's edge, where the ingreſs happened, being very ſenſibly obſcured by a penumbra, and the limbs appearing to be united, by a kind of ligament of a conſiderable breadth. This ligament became narrower and narrower, and was at length reduced to a point, and actually broken at $7^h\ 21'\ 57''\frac{1}{2}$ mean time, or $7^h\ 24'\ 13''\frac{1}{4}$ apparent time. At $7^h\ 24'\ 23''$ apparent time, the thread of light between the edges of the Sun and Venus, which was before compleated, now appeared to me of a very ſenſible breadth, and to equal $\frac{1}{10}$th of the Planet's diameter. If I have eſtimated this breadth properly, the true internal contact muſt have happened conſiderably more than a minute ſooner. The Swediſh aſtronomers have deſcribed this appearance very nearly as I ſaw it; but according to the ac-

count * given by Mr. Mallet, the interval of time between the true and apparent ingreſs, when the limbs appeared perfectly to coincide, and when the ligament was obſerved to be broken, did not exceed 53″, according to Mr. Melander's obſervation, and amounted to 56″, according to Mr. Wargentin. This appearance, in all probability, is occaſioned by the refraction which the rays of the Sun ſuffered in paſſing through the high and denſe atmoſphere of the Planet, and was perhaps rendered more ſenſible by the vapours near the horizon; as a ſimilar appearance was obſerved at the ſecond internal contact, in 1761, at very conſiderable altitudes, though in a ſmaller degree. But it will, I fear, occaſion a much greater uncertainty in the quantity of the Sun's parallax deducible from theſe obſervations, than was reaſonably expected.

By a mean of ſix obſervations, I found the Planet's diameter =58″,1; being not greater than 59″,0 from four of the obſervations, all agreeing preciſely to the ſame part of a ſecond; nor leſs than 56″,9 by the leaſt of the other two.

About fifteen minutes after the internal contact, a very thick and black cloud, which moved towards the eaſt, with a ſlow motion, along the ſkirts of the horizon, prevented any further obſervations.

The next morning, the ſky being perfectly clear, and the limb of the Sun undulating but in a ſmall degree, I made the following obſervations of the Sun's eclipſe.

* Phil. Tranſ. 1766, p. 77.

	App. Time.					
	h	′	″		′	″
At	18	33	45	Beginning of the eclipſe.		
		36	4	The double ſpot (*a*) immerges.		
		36	15	covered.		
		46	circ.	A large irregularity on the Moon's edge appeared on the Sun's diſk.		
		54	44	The Moon touches the hazineſs ſurrounding the largeſt ſpot (*b*).		
		55	15	Nucleus of the ſpot (*b*) immerges.		
		56	11	D° covered.		
	18	57	7	Hazineſs of (*b*) covered.		
	19	5	12	The Moon touches the hazineſs ſurrounding the ſpot (*c*).		
		6	14	The nucleus immerges.		
		6	38	covered.		
		8	14	The ſpot (*d*) covered.		
		10	14	The ſpot (*f*) covered.		
		10	28	The ſpot (*g*) covered.		
		16	8	The ſpot (*b*) immerges.		
	19	16	32	The ſpot (*b*) covered.	′	″
	19	17	51	Lucid parts meaſured	15	15,7
		18	37		15	12,7
		19	11		15	9,5
		19	58		15	1,5
		20	52		14	53,0
		21	28		14	49,7
		22	12		14	44,5
		22	48		14	41,3
		23	38		14	40,4
		24	28		14	39,5

App.

	App. Time.					
	h	′	″		′	″
At		25	8		14	44,6
		25	44		14	48,3
		26	31		14	49,8
		27	19		14	50,5
		28	19		14	54,5
		28	58		14	59,1
		29	38		15	1,9
		30	15		15	4,7
	19	52	37	The ſpot (*c*) totally uncovered.		
		55	17	(*b*) begins to emerge.		
		55	39	(*b*) totally uncovered.		
	20	15	30	(*i*) totally uncovered.		
	20	18	36	Eclipſe nearly ended.		
	20	18	42½	Eclipſe ends.		

The ſpots are marked with letters in the order in which they were covered by the Moon.

Many irregularities were obſervable upon the Moon's limb; though none of them were ſo pointed as ſome which I obſerved in the eclipſe of the Sun, on Auguſt 16, 1765.

Towards the end of the eclipſe, the ſky began to be hazy; which hazineſs increaſed, and was very conſiderable at ten o'clock in the morning.

On the top of New College Tower, the Reverend Mr. Lucas, Fellow of New College, with an excellent acromatic teleſcope of 6 feet, magnifying 60 times, was certain that the external contact of Ve-

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nus with the Sun was paſſed at $7^h\ 6'\ 12''$ apparent time, having perceived a ſmall impreſſion upon the Sun's edge ſeveral ſeconds ſooner; and the Reverend Mr. Clare, Fellow of St. John's College, with the ſame inſtrument, judged the thread of light to be completed at $7^h\ 24'\ 28''$, having obſerved the limbs to be in contact ſeveral ſeconds ſooner.

The next morning Mr. Lucas obſerved the beginning of the Sun's eclipſe at $18^h\ 33'\ 47''$, and the end at $20^h\ 18'\ 37''$.

Mr. Sykes, of Brazen Noſe College, with an acromatic refractor of $3\frac{1}{2}$ feet, made by Mr. Dollond, firſt ſaw Venus upon the Sun at $7^h\ 6'\ 0''$, and obſerved the thread of light to be completed at $7^h\ 24'\ 22''$.

Mr. Shuckburgh, of Balliol College, obſerved there the external contact of Venus with the Sun at $7^h\ 6'\ 8''$ apparent time, and the internal contact at $7^h\ 24'\ 25''$; though at $7^h\ 23'\ 16''$, he judged that the center of the Planet was removed more than its own ſemidiameter from the Sun's limb, or that the true internal contact was then actually paſt. He is of opinion that the obſervation of the completion of the thread of light could not be made nearer than to $8''$ or $10''$, on account of the undulation of the limbs: and he farther adds, that when Venus was wholly entered upon the Sun, he could no longer perceive the penumbra that attended the Planet before the apparent contact; but that in the room of it there appeared a ſmall circle of light, ſomewhat more luminous than the ſurrounding parts of the Sun. Mr. Shuckburgh alſo obſerved the beginning of the eclipſe at $18^h\ 33'\ 51''$,

and the end at $20^h\ 18'\ 38''$, with the appulfe of the Moon to feveral of the fpots.

In an unfurnifhed room of the Hofpital, that commanded the north-weft part of the horizon, Mr. Nikitin of St. Mary Hall, and infpector of the Ruffian gentlemen fent here for their education by the Emprefs of Ruffia, and Mr. Williamfon, of St. Alban Hall, both well verfed in the Mathematics, made the following obfervations of the tranfit, with a reflector of 10 inches, and a refractor of 8 feet:

	1ft ext. cont.			Ingrefs.		
	h	′	″	h	′	″
Mr. Nikitin	7	6	44	7	24	$15\frac{1}{2}$
Mr. Williamfon	7	6	29	7	24	$10\frac{1}{2}$

The tranfit and the eclipfe were alfo obferved here by the Reverend Mr. Horfley, F. R. S. and Mr. Cyril Jackfon, A. B. and ftudent of Chrift Church. But as Mr. Horfley propofes to lay the obfervations before the Society, I have only to add, that I believe them to have been made with all the accuracy and care that the circumftances of the time and place would permit; and that thofe gentlemen are not lefs diftinguifhed by their zeal for aftronomical and mathematical inquiry, than for their extenfive knowledge and erudition.

The latitude of Oxford is $51°\ 45'\ 15''$, as determined by myfelf, from feveral obfervations of the Pole Star, both above and below the Pole, with an excellent mural quadrant, of 32 inches, made by Mr. Bird; the focal length of the telefcope being

34 inches.

34 inches. I am the rather induced at prefent to mention this, as the latitude of Oxford, given by Mr. De la Lande in the *Connoiſſance des Temps*, and attributed to me, was determined by the late Profeffor Blifs, from obfervations made with a fmaller and lefs perfect inftrument. The longitude of Oxford is 5′ 3″ or 5′ 4″ to the weft of Greenwich, the former quantity being deduced frow a comparifon of the Sun's eclipfe, obferved by myfelf, with Mr. Short's obfervation, an allowance being made in the computation for the figure of the earth, in the effect of the Moon's parallax.

Oxford, June 14,
1769.

Thomas Hornsby.

XXIV. Venus *observed upon the* Sun *at* Oxford, June 3, 1769: *By* Samuel Horsley, *LL.B. Rector of* St. Mary, Newington, *in* Surrey, *F.R.S.*

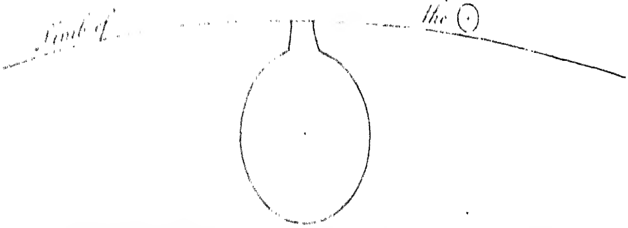
Read June 15, 1769.

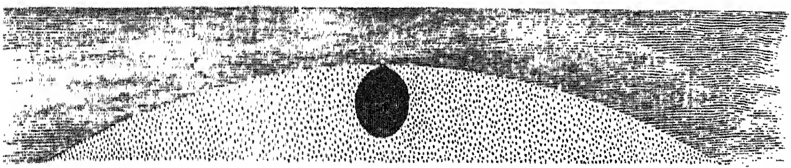
MY regulator was moved to the place of observation on Wednesday evening, and set a going on Thursday; and, between that time and nine o'clock on Sunday morning, many comparisons were made of it with Mr. Hornsby's observatory clock, by which its rate of going and difference from Mr. Hornsby's clock, at the time of observation, were pretty well determined. At 10′ before seven, by my regulator, I began to observe and to count the seconds, and about 3′ and 3 or 4″ after seven, I descried a very small black notch on that part of the Sun's limb where I expected the Planet; but it was then so small, that I was in doubt whether it was any thing more than an appearance occasioned by the horizontal vapours, which were more copious than I could have wished, and made the Sun's edge, as usual, appear ragged in many parts. But by 5′ after seven, this notch was grown so large, that no doubt remained with me that it was the Planet.

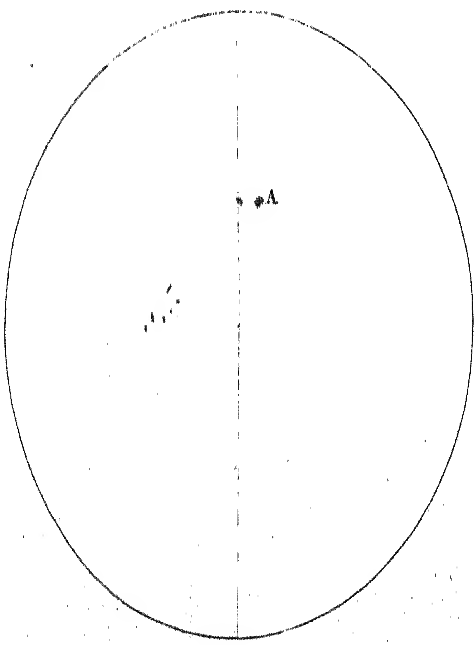
This was my observation of the external contact, which I write, however, chiefly by recollection; for, having had no experience of this observation before, not

not having obſerved the tranſit of 1761, I had conceived a prejudice that it would not be poſſible to obſerve the external contact with any accuracy, and therefore I neglected to make any other minute of what I ſaw of it, but that I was certain that the Planet was upon the Sun by 5′ after ſeven, by my regulator. Mr. Cyril Jackſon, a ſtudent of Chriſt Church, who obſerved in the ſame room with me, told me, when all was over, that he thought he had notice of the Planet's approach, by a more vehement undulation in that part of the Sun's limb where the Planet entered than in any other, which he perceived a very ſhort time before he ſaw the Planet. I confeſs that I was not ſenſible of this circumſtance. I obſerved with an 18 inch reflector; Mr. Jackſon uſed a refractor of Mr. Dollond's of nine feet. The wind was high, and very troubleſome to both of us, by the motion it gave to our inſtruments.

When the Planet had been ſo long upon the Sun's limb, and ſo large a part of its circle was plainly entered, that I thought the internal contact was near at hand, I was much aſtoniſhed to find the ſhape of the black ſpot ſuddenly altered from a large ſegment of a circle, to what I have attempted to expreſs very rudely by a ſketch, ſee TAB. VII. Fig. 1. where the lower part, which ſtill ſeemed the ſegment of a circle, is connected with the Sun's limb, by a kind of ligament of darkneſs terminated on each ſide by right lines. The ligament detached itſelf from the Sun's limb; and the light, as I thought, was viſible, all round the Planet, at $7^h\ 21'\ 52''$, by my regulator, and not earlier to my eye. And this I ſet down as the internal contact. The moment that I perceived the


Limb of the ⊙

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the ligament detached from the Sun's limb, I turned my eye to the clock, to catch the minute, and to be ſatisfied that I was right in my counting of the ſeconds. And when I returned my eye to my teleſcope, which was before or not later than the 55th ſecond, I found that the thread of light between the limb of Venus and the limb of the Sun had ſenſible breadth, and the ſhape of the Planet was perfectly circular. Mr. Jackſon reckoned the internal contact at $7^h\ 21'\ 51''$, by our regulator. He judged of it as I did, by the detachment of the ligament, which he ſaw, as well as I, from the Sun's limb. My regulator, when it was firſt ſet a-going, ſeemed to gain on Mr. Hornſby's obſervatory clock for ſome hours (the pendulum, perhaps, not being come to its natural ſwing). But on Friday evening, about a quarter after eight, it was too ſlow for Mr. Hornſby's clock 13″. On Saturday, half an hour after noon, it was 18″,9 too ſlow. And Saturday evening, at $9^h\ 30'$, it was 27″ too ſlow; and on Sunday morning, about nine o'clock, it was 32″ or 33″ too ſlow. So that at the time of the internal contact it was 25″ ½ or 26″ too ſlow for Mr. Hornſby's obſervatory clock.

I was much ſurprized, upon comparing notes with Mr. Hornſby, to find that he had judged the internal contact 14″ ½ or 15″ earlier than I did.

The foregoing narrative of what I ſaw, I have drawn up June 8th, having not converſed with any other obſervers, except Mr. Hornſby and Mr. Jackſon, and Mr. Maſkelyne, whom I met in the ſtreet this day, and talked with him very curſorily. And that my account may be purely of what I ſaw, as it

 ſtruck

ſtruck me at the time, before my own ideas were blended with thoſe of other people, and altered (as may ſometimes be the caſe by communication) from what they originally were in my own mind, I ſhall preſent this haſty memoir as I have drawn it up, without any correction or alteration.

June 8,
1769.

Samuel Horſley.

N. B. The figure that I have given does not (I believe) expreſs accurately the proportion of the ligament to the circular ſegment of Venus's diſk. I think that the right lines, which terminated the ligament, die not go off from the limb of Venus in angles quite ſo ſharp as my figure exhibits. Nor do I think their convergence was ſo great as I have drawn it.

June 10, 1769.

June 13, 1769. Since I wrote the above, I have received from Mr. Hornſby a minute of the difference of his clock from mean time, at the time of obſervation, which I forgot to bring away with me from Oxford, and therefore could only ſtate my obſervation before in the times of my own regulator and Mr. Hornſby's clock. I now ſubjoin my obſervations reduced to mean time at Oxford, reckoning Mr. Hornſby's clock too faſt for mean time by $5'' \frac{1}{2}$ at the hour of obſervation.

External

	h	′	″
External contact ——	7	3	23½
Detachment of the ligament	7	22	12½

I have likewiſe obtained from my brother, Mr. John Horſley, a minute of his obſervation made at Greenwich with an excellent refractor of Mr. Dollond's, which magnified, however, only 50 times. My brother aſſures me, that he did *not* ſee the ligament which I have deſcribed, though it was ſeen by Mr. Maſkelyne and by others, at Greenwich. He has ſet down, however, two different dates of the total ingreſs. One, which he calls cloſe contact without any light, appearing between the limbs of Venus and the Sun, at $7^h\ 28'\ 15''$, apparent time at Greenwich. Another, which he marks thus, "a thread of light, "fine as you can imagine, appearing between," at $7^h\ 29'\ 28''$. Here is an interval of $73''$ between the cloſe contact and the appearance of light. The time of the appearance of the light being reduced to mean time, and to the meridian of Oxford (reckoning the meridian of Oxford $5'\ 4''$ weſt of Greenwich, as it is ſtated in Mr. Maſkelyne's Tables), was $7^h\ 22'\ 9''$, which is only $3''$ earlier than my obſervation of the detachment of the ligament. Now from hence I conclude, that the magnifying power of the teleſcope, which my brother uſed, was too ſmall to *ſhew him the ſhape* of the ligament, yet the ligament had its effect with reſpect to obſtructing the Sun's light, which he perceived about the ſame time as others, who uſed glaſſes of greater force; which ſeems to be a ſtrong confirmation of the *reality* of what we ſaw: or that there actually was a part of the Sun's diſk, which

remained obſcure (from what cauſe I do not at preſent enquire) for ſeveral ſeconds after the limbs of the Planet and the Sun were ſeparated. I think this worthy of remark, becauſe I hear that the appearance of the ligament, which I have deſcribed, has been imputed by ſome to an inaccurate adjuſtment of the glaſſes to the obſerver's eye.

S. Horſley.

In the foregoing Paper, I have given ſeveral compariſons of my clock with Mr. Hornſby's. Its difference from Mr. Hornſby's, by a mean of all the compariſons, will be found $25''\frac{1}{2}$, at the time of obſervation. But I rely chiefly on the compariſons of Saturday night and Sunday morning, which make the difference $26''$.

XXV. *Obſer-*

XXV. *Observations of the last Transit of* Venus, *and of the Eclipse of the* Sun *the next Day; made at the House of* Joshua Kirby, *Esquire, at* Kew*. *By* John Bevis, *M. D. F. R. S.*

Read June 15, 1769.

IN the morning of June 2, 1769, I fixed my equal altitude instrument, and carefully rectified it; and, applying the proper correction to the fore and afternoon's corresponding altitudes of the Sun, I found that Mr. Kirby's clock, whose rate of going was well regulated to mean solar time, at noon was 2′ 5″ before the mean time; whence I deduced the apparent times of my observations. June 3, in the evening, I was alone in a room where I had a very commodious view of the Sun. My telescope was a very good reflector, of about three feet and a half focal length, with an aperture of near six inches, and a magnifying power of 120 times; it was steadily supported, and governed by rack-work, and I had a stop-watch in my hand. Mr. Kirby at the clock.

* Mr. Kirby's house is exactly 4″¾ of time east of his Majesty's domestic observatory, and 1′ 14″ west of the Royal observatory at Greenwich.

App. time.				
d	h	′	″	
June 3,	7	9	59	I perceived a ſudden boiling or tremor at the very ſummit of the Sun's limb, very different from what we uſually call an undulation of his limb: 8
		10	7	or 9″ after which, I called out *now!* upon diſcerning, at the ſame place, a very ſmall indentation of Venus. I think I may put the external contact about 3″ ſooner.
		28	8	The Planet ſeemed quite entered upon the diſk, her upper limb being tangential to that of the Sun: but, inſtead of a thread of light, which I expected immediately to appear between them, I perceived Venus to be ſtill conjoined to the Sun's limb by a ſlender kind of tail, nothing near ſo black as her diſk, and ſhaped like the neck of a Florence flaſk.
		28	17	The ſaid tail vaniſhed at once, and, for a few ſeconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower limb, ſomewhat like the

the leſſer end of an egg, but ſoon reſumed its rotundity.

In a few minutes more the whole circumference of Venus became very ill defined, and beſet with aſperities, which I have repreſented, as well as I could, in my Figure (TAB. VII. fig. 2.). Theſe were amazingly agitated by a ſort of curling, quick motion not eaſily to be deſcribed. A gentleman of my acquaintance fancied Venus, in this circumſtance, to reſemble a black wafer on the head of a beaten drum. In the tranſit of Venus, in 1761, which I obſerved at Savile-houſe, I ſaw not the leaſt of ſuch appearance at the exit. The Planet was then perfectly circular and well defined.

The ſky, though for the moſt part of the day clouded over, was all this while very fine.

d	h	′	
18	36	16	The Sun's eclipſe began, perhaps, 2 or 3″ ſooner.
	59	24	The ſpot A biſected. (See fig. 3.)
20	22	33	The eclipſe ended, very exact.

J. Bevis.

XXVI. *A Letter*

XXVI. *A Letter to the Aſtronomer Royal, from* John Canton, *M. A. F. R. S. containing his Obſervations of the Tranſit of* Venus, June 3, 1769, *and of the Eclipſe of the* Sun *the next Morning.*

London, June 9, 1769.

SIR,

Read June 15, 1769.

I TAKE the liberty of ſending you, incloſed, my obſervations of the tranſit of Venus, and of the Sun's eclipſe; which, if you think proper, you may lay before the Royal Society. Thoſe of Venus were made under the diſadvantageous circumſtances of being at the top of a houſe, and ſeeing through ſmoke; however, I hope they will not differ very widely from yours. I was juſt prepared to find the difference of declination between the Sun and Venus by your method, which appears to me to be a very good one, when the Sun was covered by a cloud, and I ſaw it no more. The magnifying power of the teleſcope I made uſe of was 95.

About half a minute before the total ingreſs, when the bright cuſps of the Sun were at ſome diſtance from each other, there appeared a faint light between them, a little lower than the cuſps, or nearer

to

to the center of the Planet: this I obſerved to increaſe till the time of the internal contact; which fully convinced me that there is an atmoſphere about Venus.

I had the good fortune to take ſeveral correſpondent altitudes of the Sun on the day of the tranſit, and alſo on the day before.

The longitude of Spital Square, weſt of the Royal obſervatory, I formerly found by Rocque's ſurvey, to be $16''\frac{3}{4}$ of time; and lately, by obſerving with you the exploſions of rockets, it was found to be $17''\frac{1}{10}$. I therefore add $17''$ to my time, to bring it to yours.

I am, Sir,

Your humble ſervant,

John Canton.

Spital Square, June 3, 1769.

OBSERVATIONS OF THE TRANSIT OF VENUS.

	h	′	″	
1ſt external contact at	7	8	$28\frac{1}{2}$	mean time.
1ſt internal contact at	7	26	$59\frac{1}{2}$	
Duration of the ingreſs		18	31	
		′	″	
Equation of time		2	$15\frac{3}{4}$	add
	h	′	″	
1ſt external contact at	7	10	$44\frac{1}{4}$	apparent time.
1ſt internal contact at.	7	29	$15\frac{1}{4}$	

	′	″
The diameter of the Sun, from 3 obſervations, was	31	$35\frac{1}{2}$
of Venus, from 4 obſervations,		59

At $7^h\ 38'\ 31''$, apparent time, the right aſcenſion of ♀ was greater than that of the ☉ by $8'\ 7''$.

OF THE SUN's ECLIPSE.

	h	′	″	
The beginning at	18	36	40	mean time.
End at	20	21	7	
Duration	1	44	27	

	′	″
Equation of time	2	10¾

	h	′	″	
Beginning at	18	38	50¾	apparent time.
End at	20	23	17¾	

	°	′
Digits eclipſed	6	14½

	h	′	″		dig.	′	
At	19	20	45¾	apparent time.	6	1½	eclipſed.
	19	52	45¾		4	37¼	
	20	—	44¾		3	33½	

XXVII. An

XXVII. *An Account of ſeveral ſepulchral Inſcriptions and Figures in Bas-relief, diſcovered, in* 1755, *at* Bonn *in* Lower Germany. *In a Letter to* James Weſt, *Eſquire, Pr. R. S. from* John Strange, *Eſquire, F. R. S.*

SIR,

Read Nov. 9, 1769.

YOUR diſtinguiſhed taſte for antiquities, and the zeal you have ever ſhewn in the preſervation of them, encourage me to addreſs to you the following account of ſome curious remains of Roman antiquity, found a few years ſince at Bonn in Lower Germany.

In a journey to Italy, in the ſummer of 1757, paſſing through Germany and the Tyrol, I had opportunities of collecting ſeveral ancient Roman inſcriptions, which, upon examination afterwards, I found had never been publiſhed. Upon my arrival in Tuſcany, I therefore communicated them to ſuch, among my literary friends, as delighted in theſe ſtudies, and my German inſcriptions were ſoon after publiſhed by Dr. Lami, profeſſor of theology at Florence (1).

(1) Novelle Letterarie di Firenze, t. xxiv. p. 527.

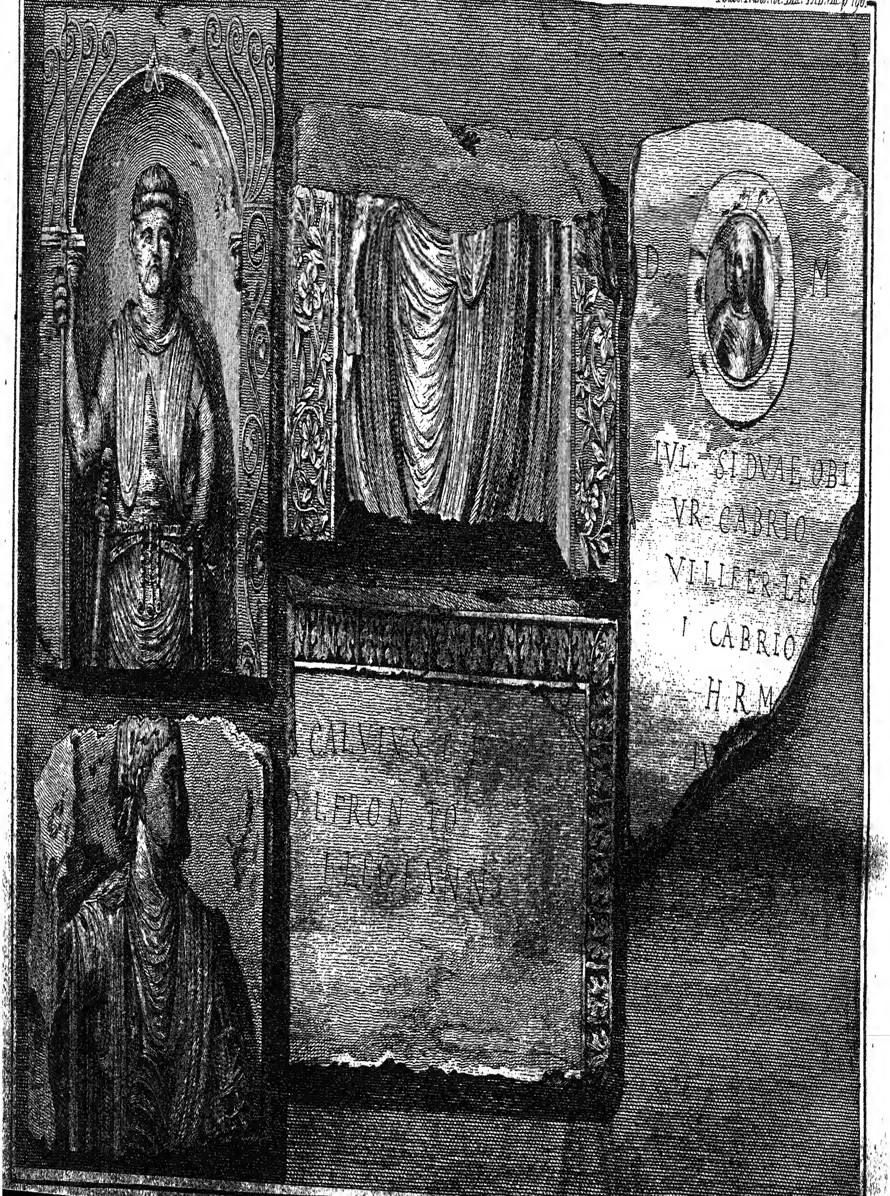
The remainder were inſerted by Abbé Donati, of Lucca, in a new collection of ancient Roman inſcriptions which he has lately publiſhed by way of ſupplement to Muratori's Theſaurus. But as ſeveral of the inſcriptions, which I had collected in Germany, were copied from bas-reliefs, which appeared to me to be curious, and very little known, I thought proper to have drawings of them made, and take this opportunity of communicating them to you; deſiring at the ſame time, that you would be pleaſed to preſent them, in my name, to the learned Society over which you preſide, ſhould you think they merit their attention and acceptance. Permit me at preſent to give you the following ſhort account of the originals.

About May, 1755, in digging ſome foundations in a garden belonging to his ſerene Highneſs the Elector of Cologne, at Bonn in Germany, ſeveral ancient Roman ſepulchral ſtones were found. Eight of theſe, being thought curious on account of the bas-reliefs and inſcriptions carved upon them, were ſoon after fixed up againſt the wall of an inner open court of the electoral palace at Bonn, where they ſtill remain.

The drawings(2), which I have now the honour to ſend you, are faithful copies of theſe antiquities, which being in the higheſt preſervation, and the inſcriptions upon them containing nothing more than the uſual form, it would be impertinent to enter into any particular deſcription of them. I cannot however omit remarking the ſingular barbarity of the Roman ſoldiers names in theſe inſcriptions. It will be further

(2) See Tab. VIII. and IX.

5 ſatiſ-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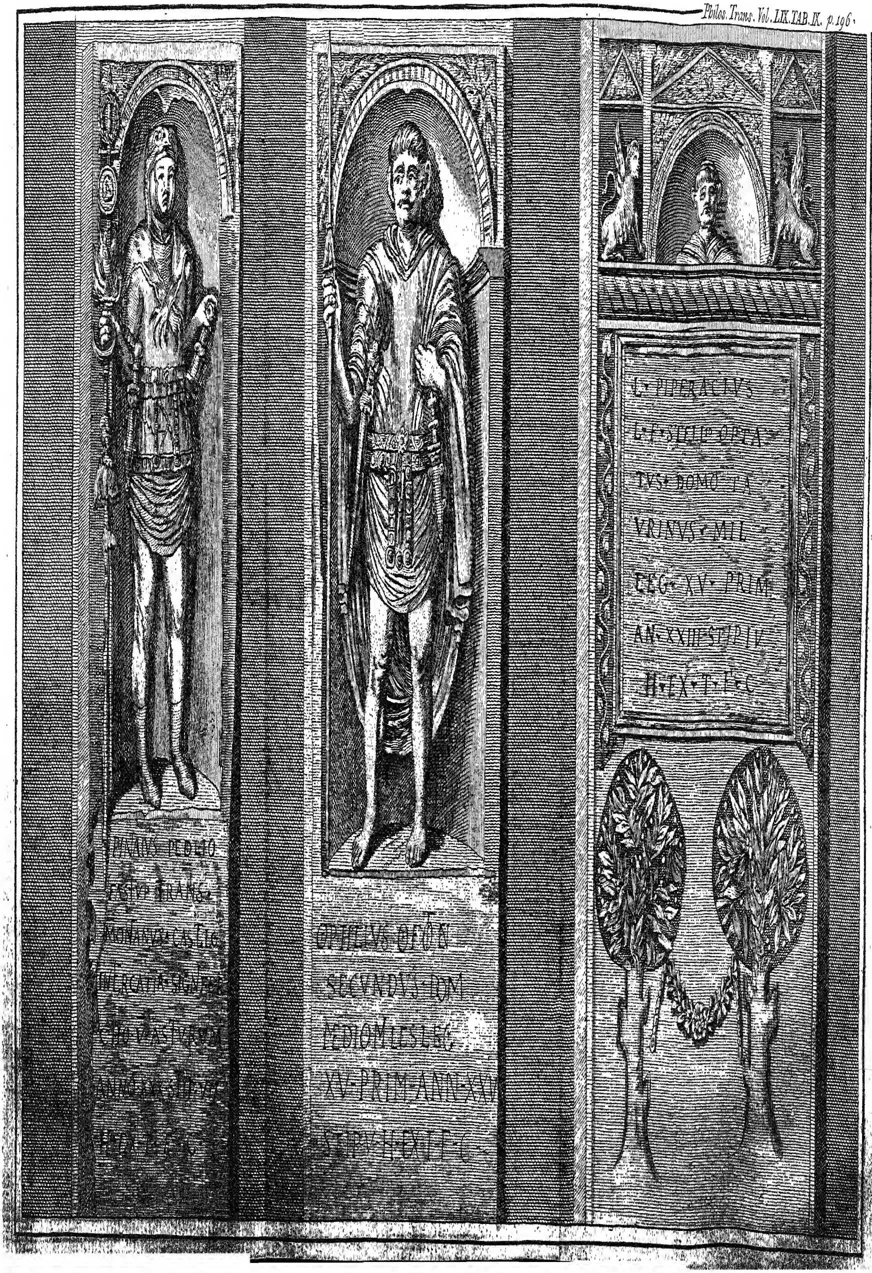
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Received June 19, 1769.

XXVIII. *An Account of the Lymphatic System in Amphibious Animals. By Mr.* William Hewson, *Lecturer in Anatomy: In a Letter to* William Hunter, *M. D. F. R. S. and by him communicated to the Society.*

SIR,

Read Nov. 9, 1769.

AGREEABLE to the promise which I made, in a postscript to the paper on the lymphatic system in birds, which the Royal Society did me the honour of publishing in their last volume, I now take the liberty of begging of you to present to the learned Society an account of the same system in a turtle. I should likewise have added a figure of that animal, had not these vessels agreed in so many particulars with those in birds, that I flattered myself the description would be intelligible without it.

This system in a turtle, like that in birds, consists of the lacteals and lymphatics, and their common trunks, or the thoracic ducts. It agrees likewise with that in birds, in not having any visible lymphatic glands either on the mesentery or near the thoracic ducts; but

differs from that in birds, in not having any glands upon the larger lymphatics of the neck; at leaſt I am inclined to believe ſo, from not having ſeen any in the diſſection of one animal of this ſpecies in which I looked for them. It likewiſe differs from that in birds in another circumſtance, to be taken notice of hereafter. Whether it agrees with the ſame ſyſtem in birds, in the tranſparency and want of colour in the chyle, I cannot take upon me to determine, as I did not ſee any of that fluid in this ſubject (*a*).

The following deſcription I took from the animal, after I had injected the larger branches of this ſyſtem with a coloured wax, and the ſmaller with quickſilver. To avoid my being miſunderſtood, when I ſpeak of the ſituation of the different parts, I ſhall mention, once for all, that the deſcription was taken from the ſubject as it lay upon its back; thoſe parts being called higheſt which were neareſt the head, thoſe loweſt which were neareſt the tail, thoſe poſterior which were neareſt the back, and thoſe anterior neareſt the belly (*b*).

The lacteals accompany the blood-veſſels upon the meſentery, running by their ſides, and communicate frequently acroſs thoſe veſſels. Near the root of the meſentery they anaſtomoſe, ſo as to form a net-work, from which ſeveral large branches go into ſome conſiderable lymphatics lying on the left ſide of the ſpine.

(*a*) In a crocodile, which I lately ſaw by the favour of Mr. John Hunter, the chyle was white.

(*b*) The animal, from which I took this deſcription, was large, meaſuring from the lower to the upper part of the ſhell two feet ſeven inches, and two feet two inches from ſide to ſide.

These

Thefe laft can be traced downwards almoft to the *anus*, and belong to the parts fituated below the mefentery, and particularly to the kidneys. At the root of the mefentery, on the left fide of the fpine, the lymphatics of the fpleen join the lacteals, and immediately above this union a fort of *plexus*, or net-work, is formed, which lies upon the right *aorta* (for there are two *aortæ* in this animal). From this *plexus* a large branch arifes, which paffes behind the right *aorta* to the left fide, and gets before the left *aorta*, where it affifts in forming a very large *receptaculum*, which lies upon that artery. From this *receptaculum* arife the thoracic ducts. From its right fide goes one trunk, which is joined by that large branch which came from the *plexus* to the left fide of the right *aorta*, and then paffes over the fpine. This trunk is the thoracic duct of the right fide; for, having got to the right fide of the fpine, it runs upwards on the infide of the right *aorta*, towards the right fubclavian vein. And when it has advanced a little above the lungs, or within three or four inches of the fubclavian vein, it divides into branches, which, near the fame place, are joined by a large branch that comes up on the outfide of the *aorta*. From this part upwards thofe veffels divide and fub-divide, and are afterwards joined by the lymphatics of the neck, which likewife divide into branches before they join thofe from below; fo that between the thoracic duct and the lymphatics of the fame fide of the neck a very intricate net-work is formed. From this net-work a branch goes into the angle made by the jugular vein and the lower part or trunk of the fubclavian: this branch, therefore, lies on the infide of the

jugular,

jugular, whilſt another gets to the outſide of that vein, and ſeems to open into it a little above the angle between that vein and the ſubclavian. I ſay ſeems to open, for the injection has not ſucceeded at this part ſo as to enable me to determine whether the laſt-mentioned branch really did enter or not. Into the above-mentioned *receptaculum*, the lymphatics of the ſtomach and *duodenum* likewiſe enter. Thoſe of the *duodenum* run by the ſide of the *pancreas*, and probably receive its lymphatics, and a part of thoſe of the liver. The lymphatics of the ſtomach and *duodenum* have very numerous *anaſtomoſes*, and form a beautiful net-work on the artery which they accompany. From this *receptaculum* likewiſe, beſides the trunk already mentioned, which goes to the right ſide, ariſe two other trunks pretty equal in ſize; one of which runs upon the left ſide, and the other upon the right ſide of the left *aorta*, till they come within two or three inches of the left ſubclavian vein; where they join behind the *aorta*, and form a number of branches, which are afterwards joined by the lymphatics of the left ſide of the neck: ſo that here a net-work, or *plexus*, is formed, as upon the right ſide. From this *plexus* a branch iſſues, which opens into the angle between the jugular and the lower part or trunk of the ſubclavian vein. In theſe net-works, formed by the lymphatics near their terminations in the veins, this ſyſtem in the turtle likewiſe differs remarkably from that in birds.

So much for the general deſcription of the lymphatic ſyſtem in this animal, I ſhall next add what I have obſerved as to the more minute diſtribution of its lacteals. In the firſt place, it may be obſerved, that

what knowledge we have of the minute diftribution of thofe veffels in quadrupeds has been acquired from examining them when filled with their natural fluid, the chyle; for the valves with which thofe veffels abound prevent our injecting their fmaller branches; as we do thofe of the arteries and veins of the inteftines. But in this animal, I have been fo fortunate as to force the valves, and to inject the lacteals from their trunks to their branches, fo as to fill them all around with quickfilver, in feveral parts of the inteftine. In thefe experiments I obferved, that the quickfilver was often ftopped by the valves, where the lacteals run upon the mefentery, or where they are juft leaving the inteftine; but when thofe valves were forced, and the quickfilver had once got upon the furface of the gut, it generally ran forward without feeming to meet with any obftacle. The lacteals anaftomofe upon the inteftines, fo that the quickfilver, which has got upon them by one veffel, in general, returns by another, at fome diftance. The larger lacteals, which run upon the inteftines, accompany the blood-veffels; but the fmaller lacteals neither accompany thofe veffels, nor pafs in the fame direction, but run longitudinally upon the gut, and dip down through the mufcular coat into the cellular or nervous, as it has been called, which in this animal is very thin in comparifon to what it is in the human fubject. So far I have traced thofe veffels to my fatisfaction; but what becomes of them after they have got to the cellular coat is not fo eafy to determine: in five or fix different experiments which I have made, the mercury paffed from the lacteals into the cells between the mufcular coat and the internal,

and

and ſpread from cell to cell, very uniformly, over a great part of the inteſtine, although but little force had been uſed, and although there was nothing like extravaſation in any other part of the inteſtine. Upon inverting the inteſtine after thus filling its lacteals, the mercury, on being preſſed, was, in many parts, driven into ſmall veſſels upon the internal coat, or villous, as it is called. From this it would ſeem, that this cellular net-work was a part of the lymphatic ſyſtem in this animal. It might indeed be ſuppoſed to be mere extravaſation, but that it is rather a part of the lymphatic ſyſtem appears probable from the following conſiderations. Firſt, from the regularity in the ſize of the cells. Secondly, from the little force uſed in the experiment, and from there being nothing like this appearance in the cellular membrane between the peritoneal and muſcular coats where extravaſations were as likely to happen. Thirdly, from my having been able, after inverting the inteſtine, to preſs the quickſilver from the cells into the very ſmall veſſels upon the internal coat; but I muſt confeſs theſe facts would not be ſufficient to determine whether theſe cells were, or were not a part of the ſyſtem, did not the analogy of the ſame part in fiſh clearly prove it. For in the cod, inſtead of the cellular net-work, as in the turtle, there is a net-work of veſſels (of which a deſcription ſhall be given hereafter) ſo that I have now no doubt but that thoſe cells are parts of the lymphatic ſyſtem, and that the ſmall abſorbent veſſels of the internal coat pour their fluid into this net-work, from which it is conveyed by the larger lacteals.

 XXIX. An

Received June 19, 1769.

XXIX. *An Account of the Lymphatic System in Fish. By the same.*

Read Nov. 16, 1769.

IN the foregoing paper on the lymphatic syftem in a turtle, I have made no mention of the manner of difcovering, and demonftrating thofe veffels; the reafon was, there is no difficulty in either the one or the other; for, in that animal the mefentery being very thin and tranfparent, and the lacteals pretty large, they are more readily difcovered than in any other animal; thence it happened that I faw thofe veffels in a turtle long before I difcovered them in birds and fifh, and that too by chance, and when I was not intent upon this inquiry. And fince I drew up the preceding account, I have found that Mr. John Hunter, in a defcription of the ftructure of a crocodile, had mentioned, by the way, that it had lacteals. Profeffor Monro, of Edinburgh, as I have been informed, likewife faw the lacteals in a turtle about four years ago. As it is natural for men, engaged in the fame inquiries, to be defirous of afcertaining their priority in the difcoveries, even when there is little merit in the making them, I therefore take this opportunity of doing juftice to thofe gentlemen, and at the fame time

time of mentioning that I firft faw thofe veffels, as near as I can recollect, in September or October 1763, which, as far as I know, was before they had been feen by either of them.

But although it was an eafy matter to fee thofe veffels in the turtle, yet it was far from being fo in birds and fifh; as the gentlemen of the Society will readily believe, from their having been fo often fought for in vain by fo many eminent anatomifts, particularly of this age. I may add, that the difcovery in birds did not give me fo much trouble as that in fifh, though now, fince I have feen them in the latter, I can more readily find them there than in birds or quadrupeds. After feeing them in birds, and in one of the *amphibia*, I was very defirous of determining whether fifh were, or were not provided with thofe veffels. This I endeavoured to do in the fame way that I had found them in birds, that is, by tying up the mefenteries of live-fifh; and for this purpofe I went frequently to the markets, and examined feveral fmall ones. I likewife diffected fome larger, when dead, but in vain. I next went to Brighthelmftone, where I found kingfton, or monk-fifh, a fpecies of fkate. Thefe being very large, and having a lean mefentery, feemed well fitted to my purpofe. I opened two of them alive, tied up their mefenteric veffels, and put them again into the falt water; and though one of them lived an hour, I could not obferve any lacteals either upon its inteftine or its mefentery. After this, I repeatedly examined the inteftines and mefenteries of common fkate and cod, and at laft was fo fortunate as to difcover the lacteals, and get a pipe into one of thofe veffels on the mefenteries of each of thefe

these fish; and, by injecting by this pipe, I found where the larger vessels lay; after which there was but little difficulty in tracing the whole system. I have now seen those vessels in a variety of fish, and shall give a description of them from a haddock. I shall proceed exactly in the order which I have found most convenient for tracing out the whole system for demonstration, beginning with one of its branches, which, as lying nearest the surface, must, of course, be divided before the other parts can be exposed to view. The account being taken from the fish as it lay on its back, those parts are called superior which are nearest the head; those inferior, which are towards the tail, those posterior which are towards the back, and those anterior which are towards the belly.

On the belly of the fish, exactly in the middle line, is a lymphatic, which runs from the *anus* upwards; this lymphatic belongs not only to the *parietes* of the belly, but to the fin below the *anus*. It runs up towards the head, passes between the two jugular fins, and, having got above them, it receives their lymphatics. It then goes under the *symphysis* of the two bones which form the *thorax*, where it opens into a net-work of very large lymphatics, which lies close to the *pericardium*, and almost intirely surrounds the heart. This net-work, besides that part of it behind the heart, has a large lymphatic on each side, which runs upon the bone of the *thorax* backwards, and when it has got as far as the middle of that bone, it sends off a large branch from its inside to join the thoracic duct. After detaching this branch, it is joined by the lymphatics of the pectoral fins, and

ſoon after, by a lymphatic which runs upon the ſide of the fiſh. This laſt-mentioned veſſel conſiſts of a trunk running on the ſide juſt oppoſite to the ribs, and from this trunk proceed branches on each ſide immediately under the ſkin; ſo that it has a beautiful penniform appearance. Beſides theſe branches, there is another ſet deeper ſeated, which accompany the ribs. After the large lymphatic has been joined by the above-mentioned veſſels, it receives the lymphatics from the poſterior extremities of the gills, and having now got as far back as the orbit, it next receives lymphatic veſſels from that cavity; but theſe veſſels do not belong merely to the orbit; for one of them comes from the noſe, and another from the upper part of the mouth. A little below the orbit, another net-work appears, conſiſting, in part, of the veſſels above deſcribed, and of the thoracic duct. This net-work is very complex; ſome of its veſſels lie on each ſide of the muſcles belonging to the gills, and from its internal part a veſſel goes into the jugular vein, by which veſſel the whole ſyſtem is terminated. The large lymphatic above mentioned, which lies upon the bone of the *thorax*, has likewiſe a proceſs running towards the upper part of the kidney, and receives ſome of the lymphatics of that organ.

The lacteals run on each ſide of the meſenteric arteries, anaſtomoſing frequently acroſs thoſe veſſels. The *receptaculum*, into which they enter, is very large in proportion to them, and conſiſts, at its lower part, of two branches, of which one lies between the *duodenum* and ſtomach, and runs a little way upon the *pancreas*, receiving the lymphatics of the liver, *pancreas*, thoſe of the lower part of the ſtomach, and the

the lacteals from the greateſt part of the ſmall inteſtines. The other branch of the *receptaculum* receives the lymphatics from the *rectum*, and the lacteals from the greateſt part of the ſmall inteſtines. The *receptaculum*, formed by theſe two branches, lies on the right ſide of the upper part of the ſtomach, (or the lower part of the *œſophagus*) and is joined by ſome lymphatics from that part; and alſo by ſome ſmall veſſels from the ſound, and from the gall bladder, which, in this fiſh, adheres to the *receptaculum*. The thoracic duct takes its riſe from the *receptaculum*, and lies on the right ſide of the *œſophagus*, receiving lymphatics from that part; and running up a little way (viz. about half an inch in this fiſh) it divides into two branches or ducts, one of which paſſes under the *œſophagus* to the left ſide, and the other goes ſtraight up, on the right ſide, runs paſt the upper part of the kidney, from which it receives ſome ſmall branches, and ſoon after it is joined by a branch from the large lymphatic that lies above the bone of the *thorax*, as formerly mentioned. It likewiſe, near this part, ſends a branch to join the duct of the oppoſite ſide, and then, a little higher, is joined by thoſe large lymphatics which make a net-work behind the heart, as formerly deſcribed. Theſe laſt mentioned veſſels receive the lymphatics from the anterior, or ſuperior part of the gills, and from the *fauces*. The thoracic duct, after being joined by theſe veſſels, communicates with that net-work near the orbit; where its lymph is mixed with that of the lymphatics from the poſterior part of the gills, from the ſuperior fins, belly, &c. and then from this net-work a veſſel goes into the jugular vein, juſt below the orbit. This laſt

veſſel,

veſſel, which I called the termination of the whole ſyſtem, is very ſmall, in proportion to the net-work from which it riſes; and indeed the lymphatics, at this part, are ſo large as to exceed by far the ſize of the ſanguiferous veſſels.

The thoracic duct of the left ſide, having paſſed under the *œſophagus* from the right, runs on the inſide of the *vena cava* of the left ſide, receives a branch from its fellow of the oppoſite ſide, and joins the large lymphatics which lie on the left of the *pericardium*, and a part of thoſe which lie behind the heart, and afterwards makes, together with the lymphatics from the gills, upper fins, and ſide of the fiſh, a net-work, from which a veſſel paſſes into the jugular vein of this ſide. In a word, the lymphatics of the left ſide agree exactly with thoſe of the right, as above deſcribed.

Beſides theſe veſſels, there is yet another part of the ſyſtem which is deeper ſeated, lying between the roots of the ſpinal proceſſes of the back-bone: this part conſiſts of a large trunk, that begins from the lower part of the fiſh near the tail, and, as it aſcends, receives branches from the dorſal fins, and from the adjacent parts of the body. It goes up near to the head, and ſends a branch to each thoracic duct, near the part where they come off from their common trunk.

This deſcription, though taken from a haddock, agrees, I believe, pretty exactly with the diſtribution of thoſe veſſels in the cod, whiting, and perhaps all other fiſh of the ſame ſhape.

To this general deſcription I ſhall add what I have obſerved of the more remarkable peculiarities of this ſyſtem in fiſh.

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In the firſt place, thoſe veſſels are remarkable in not having any lymphatic glands, that I can diſcover, in any part of their courſe. In this they agree with the turtle, but differ from birds, which have lymphatic glands on the veſſels of their necks.

Secondly, theſe veſſels in fiſh have no valves, ſo that it is an eaſy matter to fill them contrary to the courſe of the lymph. When I firſt obſerved this circumſtance, I imagined that, by injecting minutely thoſe veſſels, I might diſcover their very beginnings, and that I might alſo be enabled to determine, whether ſuch parts as the brain, eye, &c. whoſe lymphatics have not been yet ſeen in any animal, have, or have not, ſuch veſſels. At preſent I am not able to determine this matter, but I intend to proſecute the ſubject.

Thirdly, the lacteals in the cod (and I preſume in moſt other fiſh) are remarkable for having a beautiful net-work of veſſels between the muſcular and villous coat of the inteſtines (*c*). This net-work may be filled from the lacteals on the meſentery with the leaſt force imaginable. If mercury be injected into this net-work at one part, it ſpreads over the inteſtine; the communications in the net-work being very numerous: if the inteſtine be inverted, and the mercury ſqueezed, it is eaſily driven into the ſmall veſſels of the *villi* of the internal coat. From theſe veſſels the mercury can be ſqueezed into the cavity of the inteſtine; but not ſo eaſily as to make it clear whether they have, or have not, a valve at their beginning. In theſe circumſtances there is a ſtrong

(*c*) I have ſeen this net-work in the turbot, plaice, and cod.

analogy

analogy between fiſh and the turtle; but in fiſh it is more evident that there can be no deception as to the net-work between the muſcular and internal coats; for in them it is made up of cylindrical veſſels, and is not cellular, as in the turtle, and therefore not in the leaſt like an extravaſation: and in fiſh the veſſels on the internal coat are larger than in turtles.

Fourthly, this ſyſtem agrees with that of the turtle, in having a very large *receptaculum*, and in having the net-work of large veſſels near its termination in the ſanguiferous ſyſtem; and likewiſe in having the veſſel, which goes from the net-work into the vein, ſmall in proportion to the ſize of that net-work: ſo that the lymph muſt be lodged ſome time in thoſe parts before it is poured into the maſs of blood. In birds I alſo obſerved ſomething like this, their lymphatic ſyſtem being enlarged or varicoſe at different parts; but theſe enlargements are ſmall in proportion to thoſe above mentioned in fiſh and turtle.

As to the manner of diſcovering thoſe veſſels in a fiſh, one might naturally ſuppoſe, that when we know where the *receptaculum*, or any of the larger parts of this ſyſtem lie, it could not be difficult to find them; but the coats of theſe veſſels are ſo thin and tranſparent, that it is by no means eaſy. But the readieſt way of finding the whole ſyſtem is, to look for one of the veſſels which lie cloſe to the ſkin; as, for inſtance, that which runs up exactly on the middle of the belly of the haddock, cod, and other fiſh of the ſame ſhape. This veſſel is eaſily ſeen as it grows pretty large where it paſſes between the two jugular fins; and if a pipe be introduced, the whole ſyſtem may be filled by means thereof.

It is partly owing to the eaſe with which thoſe veſſels may be ſeen, after diſcovering where their larger branches lie, that I have not added a figure of this ſyſtem in a fiſh. Indeed it would be almoſt impoſſible to expreſs all its parts in one figure, from the numerous and intricate communications of thoſe veſſels near their termination in the common veins. But I have laid before the R. Society a haddock with its lymphatics and its blood-veſſels filled with coloured injections, to be compared with the deſcription. And thoſe that are deſirous of proſecuting this ſubject further will, I flatter myſelf, find it an eaſy matter to fill the whole ſyſtem, by attending to what I have ſaid above.

I ſhall beg leave to add one obſervation more; and that is with reſpect to the diſtribution of the lacteals on the *villi* (as they are called) of the inteſtines. From a variety of experiments, which I have made, I am perſuaded, that in animals, in general, each of the *villi* is compoſed of a net-work of lacteal veſſels, as well as of a net-work of arteries and veins. The very ingenious Dr. Lieberkühn has endeavoured to ſhew, that in the human ſubject each lacteal forms an *ampullula* or oviform veſicle, which is filled with a ſpongy ſubſtance. But from having injected thoſe veſſels, with mercury, in fiſh, turtle, and birds, I can clearly demonſtrate that each of the *villi* of theſe animals has a net-work of lacteals, and not an *ampullula* or oviform bag. And from comparing theſe obſervations with Dr. Lieberkühn's experiments, I am inclined to believe the ſtructure is ſimilar in the human ſubject. But as propoſing my arguments at preſent would not be altogether agreeable to the deſign of

of this paper, I ſhall refer that ſubject to ſome future occaſion. In the mean time, as I flatter myſelf that it may not be unacceptable to the learned Society, I have ready to lay before them a collection of preparations, which demonſtrate not only theſe facts relating to the *villi*, but others that are mentioned in this and the preceding paper. Of theſe preparations here follows the liſt.

N° 1. A turtle, with its lymphatic and its ſanguiferous ſyſtems filled.

2. A haddock, with its lymphatic and ſanguiferous ſyſtems filled with injections of different colours.

3. The lymphatics on the ſtomach of a cod, filled with mercury; the arteries with a red, and the veins with a green injection.

4. The lymphatics on the ſtomach of a turbot.

5. The lacteals on the meſentery and ſmall inteſtine of a cod. In this preparation the arteries are filled with a red, and the veins with a yellow injection, and the lacteals with mercury; and, the preparation being afterwards dried, the lacteals are ſeen to form a curious net-work of veſſels between the muſcular and the villous coat.

6. The ſame in a bottle.

7. The lacteals on the gut of a ſkate. In this preparation the lacteals are filled with mercury, the artery with a red, and the vein with a green wax.

8. The lacteals on the meſentery and inteſtines of a turbot. In this preparation the vaſcular net-work

net-work between the muſcular and villous coat is likewiſe very diſtinctly ſeen.

9. The meſentery and a piece of the inteſtine of a turtle; where the lacteals are ſeen filled with mercury, not only on the meſentery, but on the whole external ſurface of the gut, forming a beautiful net-work.

10. The ſame. In this the arteries are filled with a red, the veins with a blue injection, and the lacteals with mercury, and the preparation being afterwards dried, the lacteals are ſeen not only on the meſentery and the external ſurface of the inteſtine, but the cellular net-work between the muſcular and the villous coat is likewiſe diſtinctly ſeen.

11. The ſame, where the cellular net-work is filled all around the inteſtine.

12. Three preparations of the inteſtines of a turbot, in which the lacteals are ſeen to make a net-work on each of the *villi*.

13. Three more preparations from the ſame fiſh; where the lacteals are filled with mercury, and the arteries and veins with a coloured injection; each forming a net-work on the *villi*.

14. Three preparations of the gut of a turtle, where the lacteals on the *villi* of the internal coat are filled with mercury, and form a net-work.

15. A piece of the gut of a gooſe, in which the lacteals of the *villi* are filled with mercury; the arteries with a red, and the veins with a yellow injection. In this preparation both the

the lacteals, arteries and veins are ſeen to make a net-work on many of the *villi*.

16. A piece of the ſtomach of a cod, in which the lymphatics are filled with mercury, and very minutely. They are ſeen to go through the external coats of the ſtomach, dividing into ſmaller and ſmaller branches, without any appearance of a net-work between the muſcular and the villous coat; and as a conſiderable force was uſed in the injecting them, I am inclined to believe that the ſtomach in fiſh has not the net-work of veſſels between the muſcular and the villous coat, as the inteſtines have *(d)*.

(*d*) Theſe preparations were laid before the Society on the ſame evening this paper was read.

Received June 8, 1769.

XXX. *A Letter from Mr.* Lane, *Apothecary, in* Alderfgate-ftreet, *to the Honourable* Henry Cavendifh, *F. R. S. on the Solubility of Iron in fimple Water, by the Intervention of fixed Air.*

Alderfgate-ftreet, June 5, 1769.

SIR,

Read Nov. 23, 1769.

THE various impregnations of mineral waters have always been very difficult to explain: and whoever has read the divers, and often contradictory reafonings upon the fubject, muft clearly perceive, that there is ftill room for difcoveries in this part of natural hiftory.

You, Sir, by your accounts of fixed air, and of Rathbone-place water, related in the laft volume of Philofophical Tranfactions, have obliged the public with many additional lights on this branch of knowledge; and, from your known accuracy, and diligent purfuits in moft philofophical inquiries, the learned world has great reafon to hope for many other new and ufeful improvements. To your judgment therefore, I fubmit the following experiments; which are intended to fhew, that iron is foluble in fimple water, by

by the intervention of fixed air; and thence, that it is very probable, many different chalybeate springs sustain their metallic charge by this means only.

The solution of iron in mineral waters, especially in such as, by exposure, readily lose the property of striking a purple colour with astringent vegetables, has usually been attributed to some subtile gas, or volatile acid. Chymistry, however, does not discover any acid solvent for iron, but what has greater affinity with alcalies; and by means of which, therefore, this metal will be precipitated. Hence if any water appears, with a predominant alcali, which has also the power of tinging with galls, and, on being exposed to the open air, lets fall the iron, and loses that property; may we not conclude the metal to have been suspended by some other medium?

This, for example, is plainly the case in German Spa water, which Dr. Brownrigg has proved to abound with fixed air. Your own very curious experiments, before cited, have clearly shewn, that calcarious earths may be suspended in water by this principle of fixed air. And these have led me to examine, whether iron might not be dissolved by the same natural means.

I would not, however, be supposed to deny, that iron is frequently found united with an acid. The fact is sufficiently evinced in the pyrites and vitriolic earths. Nor can I doubt, but that these substances do largely contribute to the primary impregnation of waters, they being so readily soluble in them. But as an alcali, or absorbent earth, is often found more than sufficient to saturate the acid in mineral waters; this would effectually disengage every

 particle

particle of iron diſſolved by an acid, unleſs the metal was ſupported by ſome other menſtruum.

My endeavours, therefore, to detect this ſolvent, by experiments, are what I now beg leave to lay before you, in the order I made them.

Experiment I.

A wide-mouthed bottle, containing half a pint of diſtilled water and ſixty grains of ſteel-filings, was ſuſpended forty-eight hours over ſome diſtillers melaſſes, in briſk fermentation; ſo as to receive the fixed air eſcaping from the fermenting liquor; the ſurface of which was ten inches below the mouth of the bottle. Immediately after its removal, the clear water was decanted from the filings and ochrous ſediment.

This liquor had a briſk and ferruginous taſte, with a flavour of the melaſſes. An infuſion of galls, or green tea, ſoon changed part of it to a colour like ink. The remainder, being expoſed to the open air, preſently became turbid, threw up a party-coloured pellicle, and depoſited a yellowiſh ſediment.

The water now retained but very little power of tinging with galls; and in a few days loſt this property entirely.

Experiment II.

Fourteen ounces of coarſe ſugar, diſſolved in ſeven pints of water, were mixed with half a pint of yeaſt, in a bottle capable of holding more than twice the above quantity. One end of a bent tube was luted into this veſſel, ſo that no air might eſcape but through the

the tube; the other end was loofely inferted two inches within the mouth of another large bottle, charged with four hundred grains of iron filings, and fixty ounces of diftilled water. After remaining twelve hours in this fituation, the fugar working brifkly, an ounce phial was let down gently into the bottle, and filled. The water from the phial, with one drop of tincture of galls *, changed in a few minutes to a light rofy purple. The liquor being fhaken, and another phial-full taken up foon after, one drop of the tincture gave a deeper colour than before. In an hour and half more, after being fhaken again, the phial-full received a ftill deeper purple, from the like quantity of tincture. The bottle continuing as before near five hours longer; when the quantity of fixed air from the fermenting liquor was fuppofed fufficient to have faturated the water; the liquor appeared very turbid on being fhaken; and, after ftanding another hour, under the tube, to fettle, the whole was filtered.

Thirty ounces of the clear liquor was poured into two Florence flafks, and the remainder into phials, which were afterwards well corked. Two of thefe phials had their corks dipped into melted refin, fo as to cover the mouths of the bottles. Two others were enclofed with a pafte or lute.

Notwithftanding the above precautions to prevent the efcape of air, the liquors in each foon grew turbid, and by the next morning depofited yellow fediments.

* This tincture was made by infufing half an ounce of powdered galls in eight ounces of proof fpirit, for four days, without heat.

This water had a ſmart chalybeate taſte, ſomewhat reſembling Spa water; with a ſlight flavour of the fermenting liquor.

One drop of tincture of galls gave a roſy purple colour to a wine-pint of this water.

Syrup of violets turned it green *.

Soap leys, or even alcaline ſalts, either fixed or volatile in their natural ſtates, ſoon changed this liquor green, and rendered it turbid, whence a yellow ſediment enſued.

But neither of the alcaline ſalts, when previouſly ſaturated with fixed air, produced any perceptible alteration.

Nor did any viſible change happen on the addition of acids.

The thirty ounces of water, in the flaſks before mentioned, after being boiled twenty minutes, to expel the air, became very turbid, and let fall ſediments. The clear liquor being decanted, the remainder was paſſed through a filter, and, after drying, the paper appeared to have gained two grains and a quarter.

This ochrous reſiduum could not be again diſſolved in water, by means of fixed air; but was ſoluble in the vitriolic acid. The ſolution, diluted and filtered, received no colour from galls, until alcali was added to ſaturate the redundant acid; after which it ſtruck a purple, as in common ſolutions of iron.

* Simple diſtilled water, ſaturated with fixed air, by any means I have tried, makes no change in ſyrup of violets: and, when mixed with ſoap, does not curdle.

The liquor, decanted after boiling, neither changed colour with galls, nor ſhewed any precipitation with lime-water.

Experiment III.

A common quart-bottle was half filled with diſtilled water, to which were added an hundred grains of ſteel-filings. To theſe was introduced, by means of the bent tube, as much fixed air, obtained from a ſolution of alcaline ſalt in the vitriolic acid, as was judged ſufficient to fill the bottle. The whole being then ſhaken, with my hand over its mouth, the bottle ſtuck like a cupping-glaſs. About the ſame quantity of air being again added, the bottle, after ſhaking, had leſs adheſion than before. On repeating this experiment, a third time, with freſh air, the adheſion was ſcarcely perceptible. And after the fourth trial, a ſmall portion of air was obſerved to iſſue from the bottle. The water now gave a deep colour with tincture of galls.

This experiment was repeated with fixed air from different combinations. As alſo by paſſing this air through a veſſel of pearl-aſh, to arreſt any acid which might eſcape from the efferveſcing mixtures. But the ſolutions of iron, in all the trials, appeared to be exactly ſimilar, except ſome trifling difference in taſte and ſmell.

Experiment IV.

A bottle, with the like quantity of ſteel-filings and diſtilled water as in Experiment the Firſt, remained

in

in a room many weeks; yet although it was frequently ſhaken, and had an ochrous ſediment, it gave no colour with tincture of galls,

Experiment V.

A ſingle grain of iron in ſolution *, diluted with a pint of diſtilled water, changed to a deep blue purple, with the tincture of galls. Half a pint of the like mixture, expoſed many days in a wide-mouthed glaſs covered looſely with paper, let fall a ſlight precipitation; but its property of tinging with galls was not ſenſibly diminiſhed. The ſame quantity being boiled four or five minutes, in a Florence flaſk, became turbid, and depoſited a ſmall portion of an ochrous ſediment. The tincture of galls, nevertheleſs, gave as deep a colour to the clear liquor, as it would have done before boiling.

The foregoing experiments ſeem to prove, that iron remains quite unaffected by pure water, but may eaſily be diſſolved in it on the addition of fixed air; and that in whatſoever manner this air is generated, the event will appear the ſame. The laſt experiment ſhews, that where iron is ſuſpended in water, by an acid, neither expoſure nor boiling will deſtroy its property of tinging with galls; which is the reverſe of what we find to be the caſe with many ferruginous waters. Experiment the Second more particularly

* Iron-filings were diſſolved in diluted oil of vitriol to ſaturation; and, by experiment, one grain of the metal, with about two grains and a quarter of the acid, were found to be contained in ſixty-eight grains of the ſolution.

teaches,

teaches, that the iron, diſſolved in water by fixed air, is at leaſt equal in quantity to what is commonly aſcribed to moſt chalybeate ſprings: that this air, by which the metal is held in ſolution, is ſimilar to that elaſtic vapour, ſo often mentioned by writers on theſe ſubjects; which cannot be wholly retained by the cloſeſt corking, but, gradually eſcaping, ſuffers the ochrous matter to ſubſide. And that fixed air has greater affinity with alcalies than with iron, becauſe addition of alcaline ſubſtances, not ſaturated with fixed air, will diſengage the metal, while ſuch as are charged with this principle produce no alteration.

Theſe concluſions ſeemed to account for many particulars relating to medicated ſprings; but as all my trials had been made with iron in its metallic ſtate, which is rarely found in nature, it was neceſſary to repeat them upon this mineral in the ſtate of ore. I proceeded, therefore, to different experiments upon various ores; but did not find any of them to anſwer my expectation, except what is called iron ſand ore, which ſeems to contain a perfect iron.

This, at firſt, offered a material objection to my former inference. But, upon a little conſideration, it occurred, that waters, being firſt charged with pyritical matter, might afterwards have their acid neutralized with alcaline or calcarious ſubſtances, and the iron yet remain ſuſpended by air generated in the ſaturation. And I was the more ready to adopt this opinion, as it would explain, very naturally, the application of fixed air to this buſineſs of ſolution; which, I confeſs, had hitherto been to me ſomewhat difficult to account for. It was neceſſary, however, to examine the truth of this theory, by the

the teſt of experiment, which I did in the following manner.

Experiment VI.

To a pint of diſtilled water, mixed with one grain of iron diſſolved (as in Experiment the Fifth), were added forty grains of ſoap leys.

And to two ounces of lime-water, diluted with fourteen ounces of diſtilled water, was added a grain of iron in ſolution.

In both caſes the point of ſaturation was intended. The two mixtures inſtantly turned green, grew turbid, and let fall ſediments of the ſame colour. The liquors, being filtered, gave no tinge with tincture of galls.

Experiment VII.

A quart of water was mixed with two grains of iron, as before.

To one moiety, three grains of ſalt of tartar in ſolution was inſtilled. It firſt appeared green, ſoon changed yellow, and gave an orange-coloured precipitate.

To another portion two grains of powdered chalk being added, it preſently became ſtraw-coloured, and after continuing nine hours in a well-corked phial, was yet turbid, with a yellow ſediment.

The waters being both filtered, part of each changed to a purple, with tincture of galls. The remainder being boiled, a farther precipitation enſued, and the clear liquor no longer produced any alteration with galls.

This experiment being repeated with magnefia, and with the earth of alum, fhewed no fenfible difference.

The quantity of iron, left diffolved in the liquor, was found in proportion to the volume of air generated during the faturation.

If the quantity of alcali or abforbent earth was infufficient to faturate the acid, part of the iron would remain in the water after boiling. All which were difcoverable by the tincture of galls.

Experiment VIII.

To a pint of diftilled water, being faturated with fixed air, and containing four grains of chalk, was added a grain of iron in folution; the mixture continued pellucid.

Another grain of diffolved iron was diluted with a like quantity of water, previoufly faturated with air from a folution of pearl-afh in vitriolic acid: eight grains of falt of tartar, cryftallized with fixed air, and diffolved, were added to this mixture: a flight degree of cloudinefs enfued, but difappeared on fhaking; after which the liquor much refembled Spa-water; only it tafted ftronger of the iron. The quantity of chalk, and alcaline falt, in this experiment, was more than fufficient to neutralize the acid.

Both the above mixtures, on expofure, became turbid, threw up pellicles, depofited ferruginous fediments, and loft their power of ftriking a purple with galls.

Solutions of iron, and of its ores, in the marine and nitrous acids, as alſo pyrites diſſolved in rain-water, were ſubſtituted, by turns, inſtead of the original iron in vitriolic acid; and they all gave a purple colour with tincture of galls.

The trials were repeated with lime-ſtone, marble, iſland cryſtal, oſteocolla, and magneſia, in lieu of chalk; and with volatile and mineral alcali, replete with air, in the room of ſalt of tartar; but no material difference was obſerved.

The ſucceſs of theſe experiments compleatly anſwered my expectations. They ſatisfied me, that any acid holding iron diſſolved, and diluted with water, might not only be neutralized, but the water charged with an exceſs of alcaline or earthy matter, without precipitating the metal; and that the ſolvent, in theſe caſes, could be no other than *Fixed Air*. Since the iron remained in ſolution, only where this principle originally abounded in the water, or was afterwards generated in the ſaturation.

Thus much being determined, it ſeemed eaſy to apply the diſcovery to the more perfect analyzation of ſome waters; and to the re-production of others, by art, which ſhould exactly reſemble thoſe of natural medicated ſprings. This is a taſk I ſhould probably have undertaken at leiſure; had I not been informed, that Dr. Watſon, junior, by whoſe converſation my thoughts were firſt led to the ſubject, is already engaged in ſomething of this ſort. This gentleman ſaw many of the foregoing experiments repeated; and, as he is ſince gone to the German Spa, I dare ſay, his abilities and application will ſufficiently improve the

the opportunity: I ſhall gladly, therefore, wait the reſult of his enquiries.

To conclude then: It appears to me highly probable, that fixed air is generally neceſſary to the impregnation of mineral ſprings. That by the right knowledge of this principle, we may now ſolve moſt difficulties that have ariſen on this ſubject; and very poſſibly be able, hereafter, to imitate nature, in the formation of medicated waters. Whether my concluſions are well founded, I with pleaſure refer to your candid deciſion. And am,

SIR,

with great reſpect,

Your much obliged,

humble ſervant,

T. Lane.

Received June 19, 1769.

XXXI. *Account of ſeveral Phænomena obſerved during the Ingreſs of* Venus *into the Solar Diſc. By the Reverend* W. Hirſt, *F. R. S. in a Letter to the Aſtronomer Royal.*

Inner Temple, June 12, 1769.

DEAR SIR,

Read Nov. 16, 1769.

AGREEABLY to your deſire, I now ſend you a particular account of all the phænomena I obſerved during the ingreſs of Venus into the ſolar diſc; but, before I do this, I look upon myſelf as obliged in gratitude to return you many thanks for the kind manner in which you accommodated me with the apparatus neceſſary for the obſervation. The teleſcope I uſed, as you well know, belonged to Mr. Dunn, and was a reflector two feet in length, and magnified 55 times. Expecting the planet to enter the ſolar diſc at or near the zenith, I kept my eye conſtantly fixed at that part of the Sun a conſiderable time before the beginning of the tranſit. The firſt intimation which I had of the near approach of the planet, was by the ſudden appearance of a violent corruſcation, ebullition,

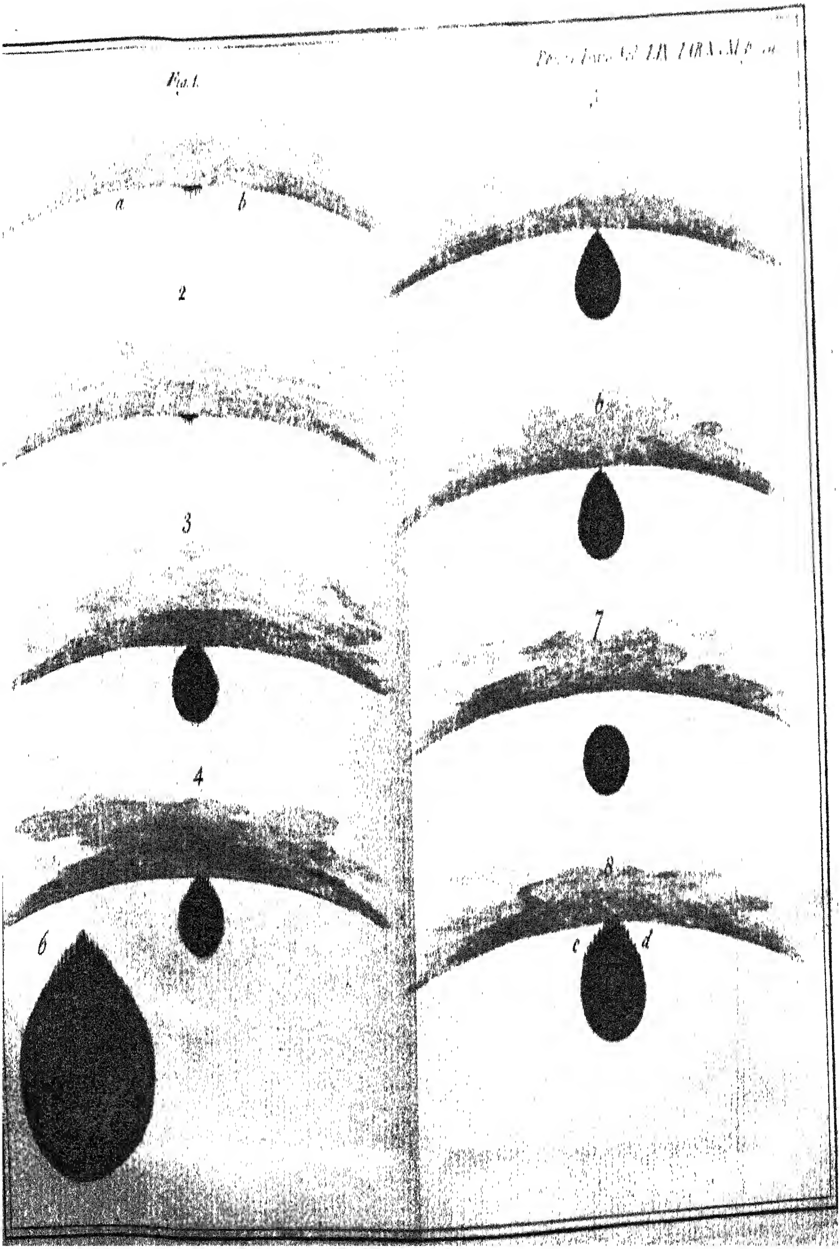

Fig. 1.
a
b
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c
d
6

tion, or agitation of the upper edge of the Sun, as in TAB. X. and XI. fig. 1. when I called out to my good friend, governour Vanſittart, who was ſo kind to give himſelf the trouble of taking the time for me, and deſired him to take care. I had not taken this precaution above five or ſix ſeconds, when I plainly ſaw a black notch breaking in upon the Sun's limb, and which ſeemed a portion of a much leſs ſphere than that of Venus, as in fig. 2. Inſtantly I deſired Mr. Vanſittart, by the word *Now*, to mark the time, which was $11^h\ 57'\ 35''$ ſidereal time, by Dr. Halley's little clock, belonging to the royal obſervatory. The laſt-mentioned time, allowing for the neceſſary corrections, and reduced to apparent time, is $7^h\ 11'\ 11''$

As I imagined, from the inſtructions of Dr. Halley, that the preciſe and accurate time of obſerving the internal contact is, when the thread of light ſhould break in between the concave edge of the Sun and the convex edge of Venus, as in fig. 6. I waited till that particular period, which was when Dr. Halley's clock marked $12^h\ 15'\ 45''$, ſidereal time, or $7^h\ 29'\ 18''$, apparent time, the difference being $18'\ 7''$ of apparent time.

The ſame phænomenon of a protuberance, which I obſerved at Madras, in 1761,* at both internal contacts, I obſerved again at this laſt tranſit: at both times, the protuberance of the upper edge of Venus diminiſhed nearly to a point before the thread of light between the concave edge of the Sun, and the convex edge was perfected, when the protuberance inſtantaneouſly

* Philoſ. Tranſ. vol. LII. part i. 1761, p. 396.

broke

broke off from the upper edge of the Sun, but Venus did not aſſume its circular form till it had deſcended into the ſolar diſc, at leaſt to the diſtance, by eſtimation, from the upper edge of the Sun, as deſcribed in fig. 7.

Although at the time of the contacts the atmoſphere was remarkably clear, yet, as the Sun deſcended towards the horizon, the atmoſphere grew more and more hazey, ſo that the edge of the Sun, as well as the edge of the Planet, began to grow more and more tremulous, and cauſed the Planet to aſſume, in appearance, different configurations, reſembling ſometimes a prolate and ſometimes an oblate ſpheroid, till we loſt ſight of the Sun by its being intercepted by a dark cloud, or rather fog-bank, ſome time before the calculated time of Sun-ſet. Although theſe laſt phænomena are entirely optical deceptions, owing to the ſtate of the air at that particular time, or to horizontal vapours, yet I beg it may be here very carefully remarked, that, at the times of the contacts, the air, as you can bear me witneſs, was perfectly clear and favourable, ſo that our obſervations were then certain, and not ſubjected to any fallacy in viſion. The following circumſtance is a proof of this aſſertion. The firſt warning which I had of the near approach of Venus to the Sun's external edge was, as I have before ſaid, by the ſudden appearance of a violent corruſcation, ebullition, or agitation of the upper edge of the Sun, five or ſix ſeconds before the edge of Venus broke in upon the Sun; where alone I obſerved the violent agitation, the edge on each ſide remaining perfectly quieſcent, as *a*, *b*, fig. 1. If this appearance had been owing to the ſtate of *our* atmoſphere only, then

then would the edge of the Sun be universally fluctuating, or trembling; but as this was not the case, the undulation must be imputed to some other cause, not improbably to an atmosphere about Venus. I am the more minute on this circumstance, because you yourself, Sir, in a late * publication, have taken notice, that when I took the observation of the transit of Venus at Madras, in the year 1761, I saw *a kind of penumbra or dusky shade, which preceded the first external contact two or three seconds of time, and was so remarkable, that I was thereby assured the contact was approaching, which happened accordingly.*

Upon your foregoing paragraph I must beg leave to make the following remark, that in the transit of this present year I did not take notice of the same phænomenon as I did of the transit of Venus in India, in the year 1761; but I must here again insist upon it, that such penumbra or dusky shade I then actually saw, but I do not recollect I then saw the least undulation, ebullition, or corruscation, as happened in the transit of this present year. Yet both phænomena were conducive to the same purpose, and served to give me notice of the near approach of the planet Venus to the solar disc, the event, in both instances, justifying the presage; and both appearances might be the consequences of the same cause; which cause, as I have before observed, might be nothing less than the atmosphere of Venus. I say *might be*, for I would not be understood to assert here any

* Instructions relative to the observation of the ensuing transit of the planet Venus over the Sun's disc, on the 3d of June 1769. By the Rev. Nevil Maskelyne, Astronomer Royal, p. 32.

thing

thing dogmatical, preſerving at this time the ſame diffidence in expreſſion as I made uſe of when I obſerved the tranſit of Venus in India, where I was apprehenſive, that *to * be able to diſcern an atmoſphere about a planet at ſo great a diſtance as Venus may be regarded as chimerical:* yet I may venture to ſay, that my obſervation of the tranſit of the preſent year ſeems to corroborate my aſſertion, in the account of the tranſit obſerved in India, in 1761; however, I ſhall not here peremptorily aſſign the cauſe, leaving ſuch remarks to be made by others.

On my return from India, I was glad to find I was not particular in remarking the ſtrange phænomenon of the oblongation of the orb of Venus at the time of both the internal contacts. It was with ſenſible pleaſure I have ſeen, in the † Philoſophical Tranſactions, that four aſtronomers at Upſal, in Sweden, as well as ‡ Mr. Dunn, in England, took notice of the ſame or ſimilar circumſtances. The appearance of this protuberance or ligament muſt now be univerſally confirmed, eſpecially by all obſervers of the tranſit of the preſent year, at leaſt by all ſuch who have viewed it through teleſcopes of ſufficient magnifying powers, and who have ſenſe enough to believe their own eyes, or candour enough to embrace and acknowledge conviction, *malgré* all prejudice and preconceived opinion.

Fig. 1. repreſents the firſt preſage I had of the approach of Venus to the Sun's external diſc. Fig. 2. is the appearance of the black notch, when I noted

* Philoſ. Tranſ. vol. LII. part i. 1761, p. 396.
† Ibid. p. 227. and vol. LVI. 1766, p. 72.
‡ Ibid. p. 184.

the

the time of the first external contact. Fig. 3. is the body of the planet within the solar disc, adhering to the Sun's upper edge, the thread of light not yet formed. Fig. 4. the protuberance forming, and the undulation at, *c*, *d*, very violent, better seen in fig. 8. Fig. 5. the undulation decreasing, and the protuberance forming itself into a point, the luminous filaments darting between the edges of the Sun and the planet. Fig. 6. the luminous filaments cease to move, and the upper edge of the planet is well defined. Its whole orb more opaque, but not yet divested of its oval appearance, the thread of light at *c*, *d*, is formed; and at this period I marked down the time of the internal contact. Fig. 7. the planet is restored to its circular figure.

I shall conclude this account with a few remarks I think it necessary to make on the manner in which my observation of the transit in India, in 1761, was inserted in the Philosophical Transactions. I am very much grieved, that the observations of the equal altitudes and meridional transits for regulating my time-keeper were altogether omitted. Had this not been the case, every one might have judged of the care and pains I took in that distant part of the world, as well in making several of my instruments myself, as in using them when made. What degree of confidence was to be given to my observations might be easily seen. Monsieur Pingré * would have had no occasion to lament that I did not acquaint *the world in what manner I observed the equal altitudes to determine the passage either of a fixed star, or of the Sun over the*

* Philos. Trans. vol. LIV. 1764. p. 156.

 meridian,

meridian, &c. If theſe particulars had been inſerted, there would have been no occaſion likewiſe to alter my numbers reſpecting the periods of the tranſit, ſince every aſtronomer, from the previous obſervations, might draw his own concluſions. Obſervations ought not to be rejected or ſtifled becauſe they do not entirely ſuit any adopted ſyſtem, or favourite parallactic angle. If I declared I ſaw an oblongation of the planet Venus, it ought not to be diſcredited becauſe another did not ſee it. If I gave reaſons for ſuggeſting that Venus had an atmoſphere, but had not a ſatellite, the report ſhould have been impartially ſtated, though another ſhould be of a contrary opinion.

I ſhall beg leave to extract the following paragraph from my original letter from India to Lord Macclesfield, on the ſubject of the tranſit of Venus, in the year 1761, which, for what good reaſon I know not, was ſuppreſſed, and had not the honour of a place in the Philoſophical Tranſactions; but which I am the more deſirous ſhould now be inſerted, as it tends to elucidate a matter of fact, and to render indiſputable an aſtronomical truth, only to be eſtabliſhed by thoſe who had the opportunity of ſeeing as I did, the entire paſſage of Venus over the ſolar diſc. The paragraph is as follows:

" Looking over the Philoſophical Tranſactions, " ſome time before the tranſit, I found Mr. Short " had obſerved a ſmall ſtar near Venus, which had " the ſame phaſis as that planet. This gave ſuſpicion " that Venus was attended with a ſatellite. A cor- " roborating circumſtance was added, *viz*, M. Caſ- " ſini, in his *Elements d'Aſtronomie*, mentions a like " obſervation. This I regarded as a favourable

" opportunity,

"opportunity, concluding, that if Venus had a satel-
"lite, it must be seen at its transit over the Sun's disc;
"accordingly, I gave notice of this to Captain Barker,
"of the Company's Artillery" (now Colonel Sir Robert Barker), "who took the observation at Pondi-
"cherry. I also mentioned it to the Jesuits, who
"observed at the Great Mount, about $7\frac{1}{2}$ miles S.
"50° W. of Madras, but neither of them saw any
"appearance in the least like a satellite. I also spoke
"of it to Governour Pigot" (now Lord Pigot) "and
"Mr. Call, who with myself saw not the least speck
"attending that planet; whence we may now ven-
"ture to affirm, *That Venus has not a Satellite.*"

I am,

with great respect,

DEAR SIR,

Your affectionate, humble servant,

W. Hirst.

XXXII. *Observations made at* Leicester *on the Transit of* Venus *over the* Sun, June 3, 1769. *By the Reverend Mr.* Ludlam, *Vicar of* Norton, *near* Leicester.

Read Nov. 16, 1769.

THE telescope, used for viewing the planet, was made by Mr. Dollond, with a triple object glass of $33\frac{1}{3}$ inches focal distance, and was found by experiment to magnify 54 times. The clock was firmly fixed; its pendulum rod was made of wood. The transit telescope was not accurately adjusted either to the meridian or horizon, but the transits of the Sun and of η Bootis registered below are sufficient to show the rate of the going of the clock, and the corresponding altitudes of the Sun, its error a few days before the transit of the planet; whence the necessary reduction of the time then shewn by the clock to apparent time may be easily derived.

Observations for examining the clock.

TRANSITS.

Day of the month, 1769.		Time by the Clock. First Wire.		Passage over Meridian.			Third Wire		Object
		′	″	h	′	″	′	″	
May	28	17	12	IX	18	0	18	47½	η Bootis
		Clouds		Clouds			Clouds		Sun
		Clouds		XXIII	59	1	59	50	
	29	13	19½	IX	14	7½	14	54	η Bootis
	31	56	29	XXIII	57	17½	58	6½	Sun
		58	46		59	35	0	23½	
June	1	56	39	XXIII	57	28½	58	17	Sun
		58	56		59	46	0	34	
	2	57	43½	VIII	58	32	59	19	η Bootis

Corresponding altitudes of the Sun, taken by reflection from water, with an Hadley's quadrant of six inches radius.

Sun's double alt. 79° 54′ May 29, 1769

	Time by the Clock Eastern Az.			Western Az.			Meridian		
	h	′	″						
Up. limb	VIII	28	33	III	27	39	XI	58	6
Center		30	17½		25	51½			4½
Low. limb		32	8		23	57½			2¾
Mean							XI	58	4⅓

Sun's double alt. 82° 55′ May 29, 1769

	Time by the Clock		
	Eastern Az.	Western Az.	Meridian
	h ′ ″	h ′ ″	h ′ ″
Up. limb	VIII 39 0	III 17 8	XI 58 4
Center	40 51½	15 18	4¾
Low. limb	42 42½	13 25	3¾

Mean		XI 58 4$\frac{1}{6}$
Mean of both sets	h ′	XI 58 4$\frac{7}{24}$
Cor^{n} for the interv.	7 0	7$\frac{5}{8}$
Passage over meridian		XI 57 56⅔
Equat. of time		3 3
Clock faster than mean time		59⅔

Sun's double alt. 96° 58′ June 2, 1769

	Time by the Clock.		
	Eastern Az.	Western Az.	Meridian
Up. limb	IX 27 50	II 29 44	XI 58 47
Center	29 57	27 41	49
Low. limb	32 1½	25 36	48¾

Mean	h ′	XI 58 48,25
Cor^{n} for interv.	5 0	5,65
Passage over meridian		XI 58 42,6
Equat. of time		2 28,4
Clock faster than mean time		1 11

If we suppose the clock to be 1′ 11″ faster than mean time, on June the second at noon, and to gain at the rate of 2½ seconds in a day, then at the time of the transit of the planet it was one minute and one second slower than apparent time.

At VII^{h} 6′ 0″, according to the time shewn by the clock, a small indenture appeared on the Sun's limb; the increase of it at VII^{h} 6′ 14″, shewed plainly that it was made by the expected planet.

The

The internal contact was first noted at VII^h 23′ 56″; at VII^h 24′ 8″, the divided part of the Sun's limb seemed wholly united.

The edge both of the Sun and Planet were in a continual tremor; at the internal contact the limb of the Sun seemed, for several seconds, to be alternately united and again separated by a kind of shootings of the Planet.

These observations, reduced to apparent time, give the external contact at VII^h 7′ 1″, the internal contact at VII^h 25′ 9″, the duration 18′ 8″.

The solar eclipse was observed by the same clock and telescope. It was manifestly begun at XVIII^h 34′ 26″, according to the time shewn by the clock. The ending was exactly noted at XX^h 20′ 8″. The Sun's limb appeared very well defined all the morning. These observations, reduced to solar time, make the beginning of the eclipse at XVIII^h 35′ 21″, the end at XX^h 21′ 2″, the duration 1^h 45′ 41″.

Observations made at Leicester, with an Hadley's quadrant, of six inches radius, for determining the latitude of the place.

1769		°	′
April 27	Sun's diameter on quadrantal arch		32
	on arch of excess		33
	repeated on quad. arch		32
	on arch of excess		32
	Sum of the meridian altitude of the Sun's upper limb, and its depression, when reflected by water	103	20½
	of the lower limb	102	18
29	Sun's diameter on quadrantal arch		32½

April

		°	′
1769 April 29	Sun's diameter on arch of excefs		32
	repeated on quadrantal arch		33
	on arch of excefs		32 $\frac{1}{4}$
	Merid. alt. and depreffion of Sun's upper limb, reflected by water	104	36
	of the lower limb	103	31
June 11	Sun's diameter on quadrantal arch		33
	on arch of excefs		30
	repeated on quad. arch		33
	on arch of excefs		30
	Merid. alt. and depreffion of Sun's upper limb, reflected by water	121	36$\frac{1}{2}$
	of the lower limb	120	33
15	Sun's diameter on quad. arch		33$\frac{1}{2}$
	on arch of excefs		30
	repeated on quad. arch		33$\frac{1}{2}$
	on arch of excefs		30
	Merid. alt. and depreff. of Sun's upper limb, reflected by water	122	2$\frac{1}{2}$
	of the lower limb	121	59$\frac{1}{2}$

	°	′	″
The latitude of Leicefter, deduced from the obfervations of April 27, is	52	36	21
of April 29,	52	37	3
of June 11,	52	37	35
of June 15,	52	37	12
Mean of thefe four obfervations	52	37	3
The latitude of Market Harborough, in Leicefterfhire, from the mean of feveral accurate obfervations of the Sun's image, projected into a dark room, by S. Roufe	52	28	30

XXXIII. A

XXXIII. *A Letter from* John Hope, *M. D. F. R. S. Professor of Physic and Botany in the University of* Edinburgh, *to* William Watson, *M. D. F. R. S. on a rare Plant found in the Isle of* Skye.

SIR,

Read May 4, 1769.

I SEND you, inclosed, a description of a rare plant, with a print of it of the natural size.

It was found, September 1768, in a small lake in the island of Skye, by James Robertson*, whom I had sent there in search of new or rare plants. The whole of it, except the head and top of the stalk, was under the surface of the water. Wherever the water was shallow, the bottom of the lake was covered with this plant, whose roots were so closely interwoven, that in some places large patches were torn up by the agitation of the waters, or other violence, and found floating on the surface, matted together.

The plant, when seen without its flowering stem, resembles somewhat the *Calamaria Dill. Histor.*

* Mr. James Robertson is an eleve of mine, and has been employed by the commissioners of the annexed estates to make a botanical survey of the distant parts of Scotland.

Musc. Tab. 80. At firſt ſight I fancied it to be the ſame, and that the *Calamaria* had not been found with its flowering ſtem: more careful compariſon convinced me they were different plants.

Although it differs, in many circumſtances, from the generic characters of the *Eriocaulon*, yet I am inclined to think it is the *Eriocaulon decangulare*, which has never yet been deſcribed, or properly figured.

I have the honour of being,

DEAR SIR,

Your moſt obedient ſervant,

Edinburgh, 10 April, 1769.

John Hope.

ERIOCAULON DECANGULARE.

Radix perennis, ſolida, interne alba, ex cujus parte inſeriore oriuntur plurimæ radiculæ teretes, ſimpliciſſimæ, pennæ paſſerinæ craſſitudinis, albæ, fere pellucidæ, per totam longitudinem diſſepimentis opacis, tranſverſis, ſive articulationibus interceptæ, in centro harum per totam longitudinem eſt linea quædam opaca. Extremitates deſcribere non audeo, quia in ſingulis ſpeciminibus mihi oblatis, omnes radiculæ ruptæ fuerunt. Sapor radicularum primo inſipidus eſt, levi poſtea cum acrimoniâ.

Folia Radicalia, ex parte ſuperiore radicis oriuntur plurima, ſub-erecta, ſimilia quod ad figuram externam, et ſimili modo diſpoſita ac in Agave Americana: ſeſqui-unciam longa, lata baſi, margine integerrimâ, ſenſim attenuantur in apicem acutum, pagina inferiore convexa; ſuperiore concava, nervoſa: et ſpatia, his nervis longitudinalibus definita, iterum dividuntur diſſepimentis tranſverſis, ſingulis ſingulis propriis, ut in figura.

Truncus ſcapus erectus; e centro foliorum oritur, vagina tenui membranacea, apice bifido, duas uncias longa, reſpectu nervorum et diſſepimentorum foliis ſimili, incluſus, ſubteres, fiſtulis ſeptem in ambitu conflatus, ſupra vaginam nudus, contortus.

Fructificatio androgyna in capitulo terminali, globoſo, imbricato.

Calyx communis, ſquamis plurimis, nigris, ſubrotundis, concavis, membranaceis; parte ſuperiore ciliatis; duæ ſunt ſeries florum fœmineorum in radio.

FLOS FOEMINEUS IN RADIO.

Squama, ovata, nigra, ſuperne ciliata ſenſim deſinens in unguem brevem, externe adſtat ſingulis floſculis.

Cal. prop. perianthium diphyllum, foliolis ovatis, concavis, nigris, ſuperne ciliatis, in unguem anguſtam ſenſim deſinentibus. Not. cilia ſquamarum ſunt alba.

Corolla dipetala, petalis albis, oblongis, concavis, ſenſim deſinentibus in ungues anguſtos, apice et dorſo ciliatis, et macula nigra in medio fere laminæ notatis.

Pistillum germen compreſſo-ſubrotundum, ſtylus brevis, ſtygmata duo longa, filiformia.

Pericarpium capſula compreſſo-ſubrotunda, nigra, bilocularis.

Semen in ſingulis loculis unicum, læve, diſſepimento affixum citrini coloris, ad apicem umbilicatum.

FLORES MASCULI IN DISCO PLURES.

Squama adſtat floſculis maſculis, ut in fœmineo flore.

Calix perianthium diphyllum foliolis cuneiformibus, concavis, ciliatis.

Corolla

COROLLA monopetala infundibuliformis, ore bilabiato, fimbriato.

STAMINA filamenta (quatuor?*) filiformia, longitudine, corollæ, vel longiora.
Antheræ nigræ, oblongæ.

Edinæ, Apr. 1769.

J. Hope, *M.* & *Bot. P.*

* Staminum numerum definire vix audeo.

EXPLI-

EXPLICATIO TABULÆ XII.

Fig. 1. Plantæ ut naturaliter creſcunt.
Fig. 2. Planta unica ſeorſim exhibita, ut apparuit aqua immerſa et inter lucem et oculum viſa.

a Capitulum magnitudine auctum.
b Scapi fragmentum magnitudine auctum.
c Fibrilla radicis magnitudine aucta.
d Folium tranſverſe ſectum magnitudine auctum.

PARTES FRUCTIFICATIONIS.

A Squama calycis communis.

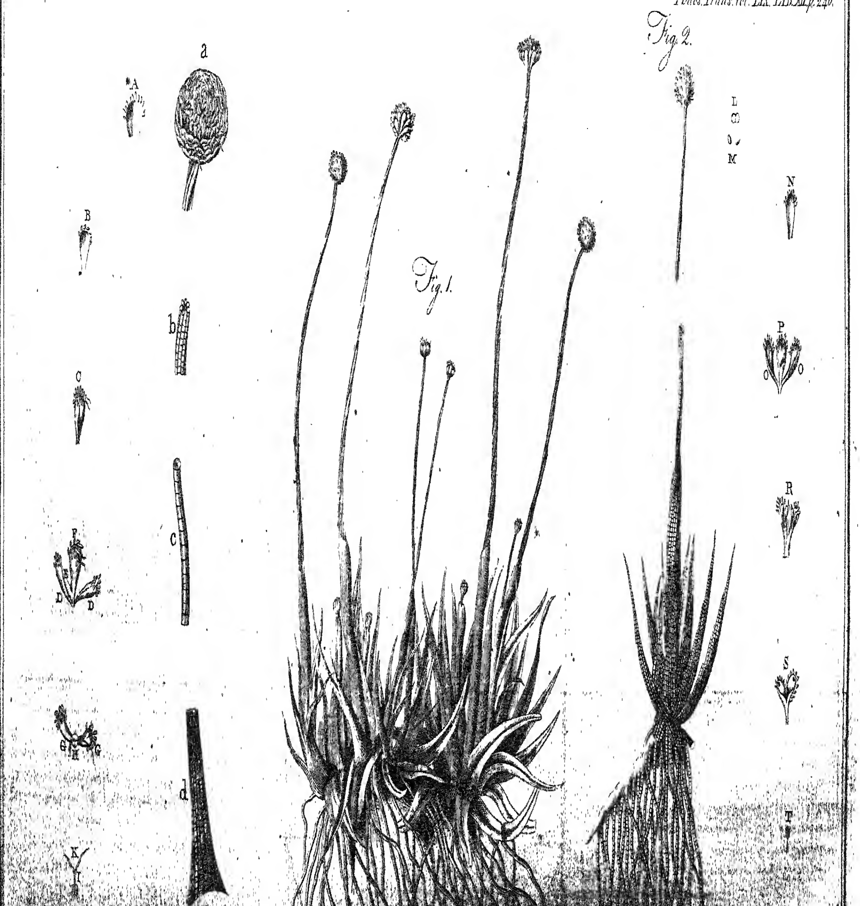
Flos fœmineus.

B Squama quæ ſingulis floſculis fœmineis adſtat.
C Foliola duo perianthii proprii floſculum fœmineum tegentia.
D D Eadem foliola perianthii proprii e ſitu naturali remota.
E Pars germinis.
F Petala
G G Eadem petala e ſitu naturali remota.
H Germen.
I Stylus.
K Stygmata.
L Pericarpium tranſverſe ſectum.
M Semina duo.

Flos maſculus.

N Squama quæ ſingulis floſculis maſculis adſtat.
O Foliola duo Perianthii proprii.
P Petalum.
R Petalum apertum ut inſertio ſtaminum appareat.
S Petalum ſtaminibus remotis.
T Stamen.

XXXIV. *Aſtro-*



EXPLICATIO TABULÆ

ERIOCAULON DECANGULARE

Partes Fructificationis

A *Squamæ calycis communis.*

B *squama quæ singulis flosculis fœmineis adstat.* C *foliola duo perianthii proprii flosculum fœmineum tegentia.* DD *eadem*

XXXIV. *Aſtronomical Obſervations made by* Samuel Holland, *Eſquire, Surveyor-General of Lands for the Northern Diſtrict of* North-America; *and Others of his Party. Communicated by the Aſtronomer Royal.*

Obſ. of Lat. MARCH 8, 1769, obſerved by Samuel Holland, Eſquire, at his houſe, bearing ſouth, 56° weſt from Quebec, diſtance from the caſtle of St. Lewis 2½ miles, with Bird's aſtronomical quadrant, the latitude, viz.

	°	′	″
Zenith angle of the Sun's upper limb	51	4	0
Deduct for the Sun's ſouthern declination	4	34	31
	46	29	29
Add the Sun's ſemi-diameter		16	9
Ditto refraction		1	42
North latitude by obſervation	46	47	20

Obſ. of Long. March 11, 1769, obſerved by the ſame at the ſame place, with Dollond's refracting teleſcope, an immerſion of the firſt ſatellite of Jupiter, at 15 hours, and 45 ſeconds, mean or equal time.

Obſ.

Obſ. of Lat. March 19, 1769, obſerved by the ſame at the ſame place, with Bird's aſtronomical quadrant, the latitude, viz.

	°	′	″
Zenith angle of the Sun's upper limb	46	45	2
Deduct for the Sun's ſouthern declination		14	42
	46	30	20
Add the Sun's ſemi-diameter		16	6
Ditto refraction			50
North latitude by obſervation	46	47	16

Obſ. of Lat. March 20, 1769, obſerved by the ſame, at the ſame place, with the ſame inſtrument, the latitude, videlicet,

	°	′	″
Zenith angle of the Sun's upper limb	46	21	16
Add the Sun's northern declination		8	58
Ditto the Sun's ſemi diameter		16	5½
Ditto the refraction		1	0
North latitude by obſervation	46	47	19½

N. B. Six more obſervations of the latitude have been taken. The mean reſult of the whole is	46	47	15

Obſ. of Long. April 3, 1769, obſerved by the ſame, at the ſame place, with Dollond's refracting teleſcope, an immerſion of the firſt ſatellite of Jupiter, at 15 hours, 10 minutes, and 22 ſeconds, mean or equal time.

Obſ. of Long. April 18, 1769, obſerved by the ſame, at the ſame place, with the ſame inſtrument, an immerſion of the ſecond ſatellite of Jupiter, at 12 hours, 39 minutes, and 36 ſeconds, mean or equal time.

Obſ. of Long. April 19, 1769, obſerved by the ſame, at the ſame place, with the ſame inſtrument, an immerſion of the firſt ſatellite of Jupiter, at 13 hours, 26 minutes, and 27 ſeconds, mean or equal time.

Obſ.

Obſ. of Long. May 28, 1769, obſerved by the ſame, at the ſame place, with the ſame inſtrument, an emerſion of the firſt ſatellite of Jupiter, at 14 hours, 2 minutes, and 40 ſeconds, mean or equal time. Alſo, obſerved by the ſame, at the ſame place, with the ſame inſtrument, a ſuperior conjunction of the fourth ſatellite of Jupiter, at 11 hours, 14 minutes, and 17 ſeconds, mean or equal time; and it entirely diſappeared at 11 hours, 24 minutes, and 3 ſeconds, mean or equal time.

Obſ. of Tranſ. June 3, 1769, obſerved, by the ſame, at the ſame place, with the ſame inſtrument, the Tranſit of Venus, as follows: at 2 hours, 28 minutes, and $1\frac{1}{2}$ ſeconds, perceived a luminous point on the lower part of the Sun's limb, by appearance; and, in the ſame place, $1\frac{1}{2}$ ſeconds afterwards, the firſt external contact was formed, which rectified as the clock or time-piece of Graham was 15 ſeconds too faſt at the time of obſervation (as proved by equal altitudes of the Sun taken with Bird's aſtronomical quadrant, on the 1ſt, 2d, 4th, and 5th inſtant) the equal or mean time of obſerving the firſt external contact will be at 2 hours, 27 minutes, and 48 ſeconds. Mr. St. Germain, of the ſeminary of Quebec, obſerved the ſame contact, at the ſame inſtant, with Short's 2 feet reflecting teleſcope. Clouds, intervening, prevented the obſervation of the firſt internal contact: but at 6 o'clock the Planet might be ſeen with the naked eye on the Sun's diſc, through the hazineſs of the atmoſphere.

Obſ. of Long. June 6, 1769, obſerved by the ſame, at the ſame place, with the ſame inſtrument, an emerſion of the firſt ſatellite of Jupiter, at 10 hours, 26 minutes, and 22 ſeconds, mean or equal time.

Obſ. of Lat. January 2, 1768, obſerved by Enſign George Sproule, of the 59th regiment of foot, on the ſouth point, at the entrance of the baſon of Gaſpée, with Hadley's quadrant, and an artificial horizon, the latitude, viz.

	°	′	″
Double angle of the meridian altitude of the Sun's center	36	38	0
Apparent altitude of the Sun's center	18	19	0
Refraction		2	41
True altitude of the Sun's center	18	16	19
	90	0	0
Sun's zenith diſtance	71	43	41
Sun's declination reduced to the meridian of Gaſpée	22	56	10
North latitude by obſervation	48	47	31

Obſ. of Lat. May 9, 1768, obſerved by the ſame, at the ſame place, with the ſame inſtrument, and an artificial horizon, the latitude, videlicet,

	°	′	″
Double angle of the Sun's lower limb, meridian altitude	117	6	0
Add for adjuſting the quadrant, error to the right,		2	10
	117	8	10
Apparent altitude of the Sun's lower limb	58	34	5
Add the Sun's ſemi-diameter		15	53
Apparent altitude of the Sun's center	58	49	58
Deduct for Refraction			33
True altitude of the Sun's center	58	49	25
	90	0	0
Sun's zenith diſtance	31	10	35
Add the Sun's declination, reduced to the meridian of Gaſpée	17	36	56
North latitude by obſervation	48	47	31

Obſ.

Obſ. of Lat. May 15, 1768, obſerved by the ſame, at the ſame place, with the ſame inſtrument, and an artificial horizon, the latitude, viz.

	°	′	″
Double angle of the Sun's upper limb, meridian altitude	121	10	0
Subtract for adjuſting the quadrant error to the left			35
	121	9	25
Apparent altitude of the Sun's upper limb	60	34	42
Subtract the Sun's ſemi-diameter		15	51
Apparent altitude of the Sun's center	60	18	51
Subtract for refraction			31
True altitude of the Sun's center	60	18	20
	90	0	0
Sun's zenith diſtance	29	41	40
Add Sun's declination reduced to the meridian of Gaſpée	19	5	50
North latitude by obſervation	48	47	30

N. B. There were 12 more obſervations made of the latitude, by the ſame perſon; but theſe are judged ſufficient to ſhew his manner of operation; but the reſult of the whole 15 make the place of obſervation 48°. 47′. 32″ north latitude.

Obſ. of Long. January 29, 1768, obſerved by the ſame perſon, at the ſame place, with Short's two feet reflecting teleſcope, an immerſion of the firſt ſatellite of Jupiter, at 14 hours, 11 minutes, and 33 ſeconds, mean or equal time.

Obſ. of Long. March 15, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an immerſion of the firſt ſatellite of Jupiter, at 14

 hours,

hours, 29 minutes, and 38 ſeconds, equal or mean time.

Obſ. of Long. March 16, 1768, obſerved by the ſame, at the ſame place, with the ſame inſtrument, an immerſion of the ſecond ſatellite of Jupiter, at 12 hours, 7 minutes, and 16 ſeconds, equal or mean time.

Obſ. of Long. March 16, 1768, obſerved an immerſion of the third ſatellite of Jupiter, at 13 hours, 38 minutes, and 18 ſeconds, equal or mean time; by the ſame perſon, with the ſame inſtrument, at the ſame place.

Obſ. of Long. April 9, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an emerſion of the firſt ſatellite of Jupiter, at 11 hours, 19 minutes, and 24 ſeconds, equal or mean time.

Obſ. of Long. April 10, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an emerſion of the ſecond ſatellite of Jupiter, at 11 hours, 38 minutes, and 45 ſeconds, equal or mean time.

Obſ. of Long. April 25, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an emerſion of the firſt ſatellite of Jupiter, at 9 hours, and 37 minutes, equal or mean time.

N. B. This obſervation is thought to be as exact as poſſible, the ſatellite emerging totally in an inſtant, and the clock being truly regulated by a number of ſingle and correſponding altitudes.

Obſ. of Long. May 9, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an emerſion of the firſt ſatellite of Jupiter, at 13 hours, 26 minutes, and 47 ſeconds, equal or mean time.

Obſ. of Long. May 12, 1768, obſerved by the ſame perſon, at the ſame place, with the ſame inſtrument, an emerſion of the ſecond ſatellite of Jupiter, at 11 hours, 11 minutes, and 34 ſeconds, equal or mean time.

Samuel Holland.

XXXV. *Obſervations made on the Iſland of* Hammerfoſt, *for the Royal Society. By* Jeremiah Dixon.

A JOURNAL of my OBSERVATIONS at HAMMERFOST.

1769. ☉ May 7	At half paſt 10 A. M. anchored in Hammerfoſt-Bay, near the town of Hammerfoſt, on Hammerfoſt-Iſland.
	In the afternoon went on ſhore, to find a proper place to obſerve in; but found none.
☽ —— 8	Went on ſhore again, to find a place; and, after much ſearch and travel, fixed upon one: but, though the beſt this or the adjacent iſlands could afford, is very difficult of acceſs.
♂ —— 9	Landed the houſe and obſervatory.
☿ —— 10	Digging holes for fixing the clock-poſt, and ſtand for the tranſit-inſtrument. Note, the ground ſo much frozen and rocky, could not finiſh them this day.

The

1769.
May 10 | The carpenters, this day, put up the houſe, ſeparate from the obſervatory; as there was not room left by the carpenter at Greenwich for fixing the tranſit-inſtrument.

♃ —— 11 | Finiſhed the digging, and fixed up the poſt for the clock; alſo put up that part of the obſervatory which moves round. Note, the poſt for the clock was 3 feet deep in the ground.

12 | Carried the inſtruments on ſhore: the carpenters working at the obſervatory.

♄ —— 13 | Fixed up the clock, and ſet it a-going. Note, The upper part of the bob of the pendulum was ſet to ſcratch, marked thus ꝝ and the index to N° 14. It vibrated on each ſide of the perpendicular 1° 30′.

☉ —— 14 | Put up the quadrant. It was placed upon the ſtand provided for it. The ſtand ſtood on a large caſk filled with water, very firmly fixed in the ground; and the box in the bottom of the ſtand filled with ſtones. Cloudy weather.

Placed one thermometer without doors, and the other within the obſervatory; alſo placed the barometer in a proper place.

Cloudy till ♄ May 20, in the evening; when I took the following equal altitudes of the Sun.

	Sun West. L. limb. h ′ ″	Sun West. Up. limb. h ′ ″	Sun East. Up. limb. h ′ ″	Sun East. L. limb. h ′ ″	Point on the quadrant.
♄ May 20	9 4 42	9 11 5	21 54 18	22 0 44	69,00
		16 26	59 37	6 5	
	15 16½	21 44	22 4 54	11 20	
	9 41 14	9 47 46 –	21 17 33	21 24 6	72,00
	46 33	53 8	22 58	29 31	
	52 2 —	58 33	28 20	34 50	
	10 6 1	10 12 40 +	20 52 24	20 59 10	74,00
	11 28	18 9½	58 0	21 4 40	
	16 58	23 42 —	21 3 27	21 10 7	
☉ May 21	10 12 13	10 18 53	These correspond to the above point 74, taken in the morning.		
	17 43	24 26			
	23 16	30 3			

From the points 72 and 74, in the above sets of observations, I find the clock on the 21st, at noon, to be 12′ 53″,2 too slow; and loses at the rate of 2″ per day of sidereal time.

☽ May 22. Marked a meridian. The mark S° is distant about 2½ miles, and that N° about 2 miles. The situation would not allow me to have marks nearer.

Put the clock forward, to be nearer the right ascension of the mid-heaven.

This afternoon came on a most violent storm of wind, hail, and snow, which continued till Tuesday evening.

♂ —— 23 Cloudy, with snow, &c.

☿ —— 24 Ditto. Fixed the transit-instrument.

♃ —— 25 }
♀ —— 26 }
♄ —— 27 } Cloudy, snow, &c.
☉ —— 28 }
☽ —— 29 }

1769 ♂ May 30	Sun L. limb. h ′ ″	West. Up. limb. h ′ ″	Sun	East.	Point. ° ′
	9 29 28 34 48 40 6	9 35 56½ 41 17 46 35			65,00
	49 46½ 55 6 10 0 21	9 56 12 10 1 29 6 49			66,40
	14 4 19 22 24 43½	10 20 32 25 53 31 12			68,40
	10 38 31 43 53 49 18½	45 3 50 27½ 55 52			70,40

	2d wire. h ′ ″	3d or middle wire. ′ ″	4th wire. ′ ″	
☿ May 31	16 34 33½ – – – – –	35 19 37 35+	36 4½ – – – –	Sun's first east limb passed the transit. Last or west limb passed at midnight.

Zen. dist. Sun's upper limb 86° 47′ 50″.

	Sun Up. limb. h ′ ″	East. L. limb. h ′ ″			
♃ June 1	23 46 5 51 28 56 50	23 52 34 57 58 0 3 20			63,00
	0 10 44 16 14 21 44	0 17 22 22 54 28 25			61,00
	0 31 46½ 27 23 43 3	0 38 34 44 14½ 49 58			59,20

1769 ♃ June 1	Sun Up. limb. h ′ ″	East. L. limb. h ′ ″	Sun	West.	Point.
	53 31 :	0 0 $33\frac{1}{2}$			57,40
	59 20	0 6 27			
	— — — — —	12 $29\frac{1}{2}$			

	Very hazy. 1st wire. h ′ ″	2d wire. ′ ″	3d or middle wire. ′ ″	4th wire. ′ ″	5th wire. ′ ″	
♀ June 2 at noon.	4 39 39	0 40 24	41 10—	41 55	42 40	Sun's W. limb p[assed] the transit.
	— — — — —	— — — — —	43 27	— — — — —	— — — —	E. limb passed d[…]

Zen. dist. of the Sun's upper limb 48° 5′ 50″.

The pendulum of the clock now vibrates 1° 20′ on the left hand, and 1° on the right.

This afternoon the Sun being pretty well defined, adjusted the focus of the telescope to my eye, as follows. — Short eye-piece and broad little specu[lum] (I. 20th. 25th.) 0 19 12 to the right of 0 on the adjusting scale. Ditto with micrometer (I. 20th.) 0 0 to the right.—Long eye-piece and narrow little speculum (I. 20th. 25th.) 0 0 7 to left of 0. Long eye-piece and broad little speculum (I. 20th. 25th.) 0 18 17 to the right.——These the means of 8 or 9 trials each.

The spot near the Sun's center was thus defined.

♀ June 2	Sun L. limb. h ′ ″	West. Up. limb. h ′ ″	Sun Up. limb. h ′ ″	East. L. limb. h ′ ″	Point. ° ′	
	10 55 1	— — — — —	— — — — —	22 21 37	70,40	
	11 00 26	11 7 2	22 20 $26\frac{1}{2}$	27 $3\frac{1}{2}$		
	— — — — —	12 28	25 $50\frac{1}{2}$	32 26		
	11 15 50+	11 22 31—	21 53 $48\frac{1}{2}$	22 0 37	72,20	Computing from point, the Sun p[assed] the north point o[f the] meridian at 16 $3''4,2$ per clock.
	21 21	28 3	59 23	6 $9\frac{1}{2}$		
	26 55	33 41 :	22 4 $56\frac{1}{2}$	11 39		

1769 ♀ June 2	Sun West. L. limb. h ′ ″	Up. limb. h ′ ″	Sun East. Up. limb. h ′ ″	L. limb. h ′ ″	Point.
	11 37 3½	43 56½	21 32 1	21 39 3+	° 74,00
	42 45	49 42	37 49	44 45	
	48 26	55 29:	43 33	50 25	

	2d wire. h ′ ″	3d wire. ′ ″	4th wire. ′ ″	
	16 41 28	43 13½	43 59½	Sun's east or first limb passed the transit at midnight.
At mid-	- - - - -	45 20½	- - - - -	Ditto west limb

Zen. dist. of the Sun's upper limb 86° 32′ 57″. Outer arc Grand Sec. Ver. 92 1 9

	Sun East. Up. limb. h ′ ″	L. limb. h ′ ″	Sun West. L. limb. h ′ ″	Up. limb. h ′ ″	Point. ° ′	
♀ June 2	- - - - -	23 10 35	10 11 30::	- - - - -	66,40	
♄ June 3	23 9 26	15 52½	16 49	10 23 17½		
	14 38	21 9	22 9	- - - - -		
	23 24 20	30 46½	9 50 20	9 57 44−	65,00	Computing from this point, the Sun passed the meridian at 4h 46′ 2″,52 per clock.
	29 37	36 3½	56 37	10 3 1		
	34 53::	41 20	10 1 58½	8 21		
	- - - - -	23 55 8	9 26 47	9 33 15½	63,00	
	23 54 00	- - - - -	32 12	38 36		
	- - - - -	0 5 53	37 33	- - - - -		
	0 13 18½	0 19 55	9 1 43	9 8 24	61,00	Computing from this point, the Sun passed the meridian at 4h 46′ 3″,48 per clock.
	18 45½	25 24	7 14½	13 49		
	24 13	30 55	12 45 Hazy.	19 20		The mean 4h 46′ 3″.

Hence the clock is 11″,13 slow of sidereal time, and loses at the rate of 6″,84 per day.

By

By comparing the inſtant of time $16^h\ 44'\ 3'',42$ per clock the Sun paſſed the meridian under the Pole on the 2d of June, with the inſtant $16^h\ 44'\ 22'',05$ per clock, it paſſed the meridian mark per tranſit inſtrument, I find it is $18'',63$ later per tranſit, that is, the mark northward ſtands $18'',63$ too much to the eaſt. Now by putting up another mark at the ſame diſtance as the firſt, ſo as to correſpond to the ſecond wire, and meaſuring the diſtance between this and the firſt mark, I find it to be 36 feet. This increaſed in the ratio of the zen. diſt. $86^\circ\ 48'$ to rad. gives 36,057 feet for the ſpace correſponding to $45'\frac{1}{2}$, the time of the Sun's paſſage from the ſecond to the third or middle wire.

The reaſon of this increaſe is, that all the wires in a tranſit inſtrument, except the middle one, deſcribe leſſer circles, parallel to the middle one, which deſcribes an azimuth.——Having got what ſpace correſponds to $45''\frac{1}{2}$, we have, by proportion, 14.76 feet for the ſpace, anſwering to $18'',63$, which the mark muſt be moved weſtward to be in the true meridian.

THE TRANSIT OF VENUS.

	Time p. clock			
	h	′	″	
1769 ♄ June 3	13	40	00	Saw the planet Venus upon the Sun about half immerged:
		43	00	Not totally immerged.
		50	00	The planet ſeemed to be compleatly upon the Sun, but no thread of light: this was an inſtantaneous view, and through a thin cloud.
				The air all this time very hazy.
	19	47	00	Saw Venus upon the Sun's limb, the 2d internal contact being paſt.
				After this, all cloudy as before.

THE ECLIPSE OF THE SUN.

Time p. clock			Micrometer			Ap. time			Value of microm.			
h	′	″	Inch.	20th	25th	h	′	″	°	′	″	
2	3	23	3	0	20	21	13	$48\frac{1}{2}$		18	51,9	The verſed ſine of light
	7	44	2	13	17		18	9		16	39,1	Ditto
	10	30	2	9	21		20	54,3		15	27,4	Ditto
	13	33	2	5	12		23	57		14	6,14	Ditto
	22	5	4	17	21		32	$27\frac{1}{2}$		30	22,8	Diſt. of cuſps
	24	57	4	19	11		35	19		30	52,7	Ditto
	30	4	1	6	14		40	$25\frac{1}{4}$		8	13,15	Verſed ſine of light
	33	37	1	4	11		43	57,6		7	33,6	Ditto

1769	Timep. clock h	′	″	Micrometer Inch.	20th	25th		Ap. time h	′	″	Value of microm. °	′	″	
♄ June 3	2	38	55	1	[illegible]	23			49	14,7		7	23,9	Verſed ſine of light
		41	3	1	[illegible]	$21\frac{1}{2}$			51	52,2		7	41,3	Ditto
		44	8	1	[illegible]	7			54	26,8		8	7,9	Ditto
		47	20	1	8	19			57	58,3		8	54,2	Ditto
		50	45	1	11	21		22	1	2,7		9	51,7	Ditto
	3	1	45	4	13				12	0,8		28	55,3	Diſt. of cuſps
		4	5	4	10	18			15	5,5		28	10	Ditto
	3	38	20						48	30				End of the eclipſe
				5	1	12	Sun's diameter 31′ 31″							
				5	1	$12\frac{1}{2}$								
				5	1	12 +								
						3	Sub. for adjuſtment							

☉ June 4 Zen. diſt. of the Sun's lower limb at noon 48° 23′

A Table ſhewing the minutes and ſeconds anſwering to the diviſions of the micrometer ſcale.

Inch.	′	″
1	6	13,08
2	12	26,16
3	18	39,24
4	24	52,32
5	31	05,40

20th	′	″	25th	″
1	0	18,654	1	0,746
2	0	37,308	2	1,492
3	0	55,962	3	2,238
4	1	14,616	4	2,984
5	1	33,270	5	3,730
10	3	6,540	10	7,460
15	4	39,810	15	11,190
20	6	13,080	20	14,920
			25	18,650

☉ June 4 | Took the dip of the ſea from the obſervatory, and found it to be nearly 21′.
Packing up the inſtruments.

☽ June 5 | Got all the inſtruments on board the Emerald.

A Table shewing the Height of the Mercury in Fahrenheit's Thermometer, v the Observatory, and without in the Shade, three Times per Day; and the H of the Mercury in the Barometer once per Day, while at Hammerfost.

1769	Within observatory.			Without observatory.			Barometer.
	Morn.	Noon.	Even.	Morn.	Noon.	Even.	
May 15	—	32	—	—	32	—	27,3
16	31	32	33	31	33	33	27,69
17	31	36	32	31	35	31	27 69
18	34	36	37	34	37	36	27,00
19	34	38	39	35	39	38	27,83
20	40	47	43	40	43	43	27,80
21	36	36	30	36	36	30	27,70
22	38	38	31	38	37	30	27,00
23	36	36	35	36	36	35	27,93
24	36	36	36	36	36	36	27,74
25	33	34	31	33	34	31	27,60
26	34	36	35	34	35	35	27,50
27	33	37	37	33	37	37	27,90
28	32	35	34	33	36	33	27,91
29	36	39	34	36	39	33	27,74
30	39	49	52	36	49	55	27,65
31	50	50	46	50	50	46	27,71
June 1	43	43	40	43	43	40	27,90
2	40	44	44	40	44	44	27,70
3	41	42	40	41	42	40	27,60
4	38	38	36	38	38	36	27,50

The Latitude of the Observatory is, per obs. June 2, at noon 70° 38′ 22′
4, at noon 70° 38′ 23′

Note, The error of the line of collimation of the quadrant did not, upon appear to be any thing worth notice.

The altitudes, northward, are so low as not to be depended upon.

The longitude of the observatory is (by comparing the observations of the ecl the Sun with those made at Greenwich),

		h ′ ″	
Per	1st measurement with the micrometer	1 34 40	These observations made before the m of the eclipse.
	2d ditto	1 34 35	
	3d ditto	1 34 44	
	13th ditto	1 35 [illegible]	These observations after the middle
	15th ditto	1 35 13	
	End of the eclipse	1 35 10	
		1 34 55 = 23° 43′ 45″	

East of the Royal observatory at Greenwich.

Jere. Di

XXXVI.

XXXVI. *Astronomical Observations made at the* North Cape, *for the Royal Society. By Mr.* Bayley.

1769 April 28, 29, and 30, got the obſervatory and dwelling-houſe built, and inſtruments on ſhore.

May 1 Set up an oak plank $4\frac{3}{4}$ inches thick, and 14 inches wide. This plank was ſet a little more than 2 feet in the ground, and well rammed with earth and ſtones ſo that it was very ſteady and firm; to which I ſcrewed the aſtronomical clock truly perpendicular (by which means it was independant of the obſervatory and its ſhaking by the wind) and ſet it going nearly with ſidereal time.

3 Set up the tranſit inſtrument nearly in the meridian.

4 Examined the line of collimation of the quadrant, and found it correct. Examined the clock, and found the pendulum to vibrate $1^{\circ}\frac{1}{2}$ on each ſide of nothing.

Here follow ſome correſponding altitudes of the Sun, from which the going of the clock is determined.

	′	″	h	′	″	′	″			Z. D. ° ′
6	27	13	23	34	37	42	22	Morning	Sun's upper limb	61,30
	20	58	6	13	30	5	48	Afternoon		
	5	57	0	14	31	23	8	Morning	Sun's upper limb	50,0
	42	6	5	33	32	24	55	Afternoon		
7	29	16:	0	38	39:	48	24:	Morning	Sun's upper limb	57,36 very hazy
	26	24:	5	16	59:	—	—	Afternoon		
	39	20:	0	49	17:	59	44:	Morning	Sun's lower limb	
	16	18:	—	—	—	—	—	Afternoon		
8	29	53	23			44	29	Morning	Sun's upper limb	61,20
	34	0	6			19	25	Afternoon		
	37	39	23	45	13	—	—	Morning	Sun's lower limb	
	26	16	6	18	44	—	—	Afternoon		
13	26	46	21	33	0	38	59	Morning	Sun's upper limb	71,20
	33	23		39	35	45	34	Afternoon		

This day ſet up a meridian poſt nearly in the meridian, by help of the quadrant, at about half a mile from the obſervatory. Examined the pendulum of the clock, and found it to vibrate $1^{\circ}\frac{1}{2}$ on each ſide of nothing.

15	—	—	1	9	25	18	54	Morning	Sun's upper limb	55,40
	—	—	5	49	26	39	54	Afternoon		
	9	59	1	19	46	—	—	Morning	Sun's lower limb	
	48	48	5	39	0	—	—	Afternoon		
18	48	56	21	55	4	1	8	Morning	Sun's upper limb	70,5
	55	31	22	1	42	—	—	Afternoon	Sun's lower limb	
20	—	—	1	36	21	46	20	Morning	Sun's upper limb	54,12
	—	—	6	1	48	51	50	Afternoon		
	37	3	1	47	16	—	—	Morning	Sun's lower limb	
	1	10	5	50	56	—	—	Afternoon		

Corresponding Altitudes of the SUN.

1769		′ ″	h ′ ″	′ ″			Z. D. ° ′
☉	21	– – –	22 6 2	12 6	Morning	Sun's upper limb	69,27
		6 28	12 38	– – –	Morning	Sun's lower limb	
♀	26	At 7^h 22′ per clock, clock stopped, but by what cause I cannot find, it not down.					
		At $9\frac{1}{2}$ set it going again, as near as I could guess, with sidereal time.					
		Pendulum vibrates $1^\circ\frac{1}{2}$ on each side of nothing.					
♃ June	1	Wound up the clock The pendulum vibrates $1^\circ\frac{1}{2}$ on each side of nothing.					
♄	3	49 45	22 55 59	2 3	Morning	Sun's upper limb	67,10
		– – –	10 34 52	28 47	Afternoon		
		56 23	23 2 34	– – –	Morning	Sun's lower limb	
		34 28	10 28 14	– – –	Afternoon		
		– – –	23 6 16	12 19	Morning	Sun's upper limb	66,50
		– – –	10 24 31	18 26	Afternoon		
		6 40	23 12 53	18 51	Morning	Sun's lower limb	
		24 4:	10 17 52:	11 56	Afternoon		
		– – –	23 49 31	55 38	Morning	Sun's upper limb	63,20
		– – –	9 41 12:	35 5:	Afternoon		
		49 56	23 56 14	02 18	Morning	Sun's lower limb	
		40 47:	9	28 24:	Afternoon		
		– – –	0 7 30	13 42	Morning	Sun's upper limb	61,54
		– – –	9 23 13:	17 2:	Afternoon		
		7 55	14 16	20 30	Morning	Sun's lower limb	
		22 48:	9 16 26:	10 12:	Afternoon		
		– – –	0 48 51	55 26	Morning	Sun's upper limb	58,42
		– – –	8 41 50	35 13	Afternoon		
		49 19	0 56 2	– – –	Morning	Sun's lower limb	
		– – –	8 34 37	– – –	Afternoon		
		– – –	1 16 46	23 47	Morning	Sun's upper limb	56,40
		– – –	8 13 51	6 50	Afternoon		
		17 14	1 24 26	– – –	Morning	Sun's lower limb	
		13 23	8 6 11	– – –	Afternoon		
		– – –	1 39 55	47 29	Morning	Sun's upper limb	55,5
		– – –	7 50 41	– – –	Afternoon		
		41 27	1 48 11	– – –	Morning	Sun's lower limb	
		– – –	7 42 29::	– – –	Afternoon		
		– – –	2 13 44	22 27	Morning	Sun's upper limb	53,0
		– – –	7 16 52	8 7	Afternoon		
		14 18	2 23 19	– – –	Morning	Sun's lower limb	
		16 18	7 7 18	– – –	Afternoon		
		– – –	2 30 20	39 56	Morning	Sun's upper limb	52,5
		– – –	7 0 16	50 39	Afternoon		
		30 56	2 40 52	51 8	Morning	Sun's lower limb	
		59 40	6 49 45	39 24	Afternoon		
		– – –	2 56 16	7 58	Morning	Sun's upper limb	50,50
		– – –	6 34 20	– – –	Afternoon		
		57 2	3 9 6	– – –	Morning	Sun's lower limb	
		33 33	6 21 31	– – –	Afternoon		
♃	8	at	11 15 52,5	a mean of four observations Z. D. ☉ L. L. = 69° 26′ 1			
♀	9	– – –	23 21 46	27 57	Morning	Sun's upper limb	67,0
		– – –	10 58 14:	52 6:	Afternoon		
		22 11	23 28 26	– – –	Morning	Sun's lower limb	
		57 49:	10 51 39:	– – –	Afternoon		

Corresponding Altitudes of the SUN.

1769	′ ″	h ′ ″	′ ″			Z. D. ° ′	
June 9	33 43	23 0 0	46 0	Morning	Sun's upper limb	65,32	
	46 18	10 40 9	34 0	Afternoon			
	40 16	23 46 34	— — —	Morning	Sun's lower limb		
	39 45	10 33 27	— — —	Afternoon			
	— — —	1 28 43	— — —	Morning	Sun's upper limb	57,0	
	— — —	8 51 10	— — —	Afternoon			
	29 12	1 36 9	42 56	Morning	Sun's lower limb		
	50 40	8 43 42	36 54	Afternoon			
10	— — —	23 28 22	34 32	Morning	Sun's upper limb	66,44	
	— — —	10 56 56	— — —	Afternoon			
	28 45	23 34 56	— — —	Morning	Sun's lower limb		
	0 0	11 0 0	— — —	Afternoon			
	— — —	0 59 30	5 50	Morning	Sun's upper limb	59,26	
	— — —	9 28 43	22 22	Afternoon			
	59 56	1 6 27	— — —	Morning	Sun's lower limb		
	— — —	9 21 44	— — —	Afternoon			
	— — —	1 20 57	27 32	Morning	Sun's upper limb	57,48	
	— — —	9 7 16	— — —	Afternoon			
	21 26	1 28 12	— — —	Morning	Sun's lower limb		
	6 47	9 0 0	— — —	Afternoon			
14	— — —	1 42 4	48 39	Morning	Sun's upper limb	57,30	
	— — —	9 27 34	21 1	Afternoon			
	42 31	1 49 18	— — —	Morning	Sun's lower limb		
	27 8	9 20 20	— — —	Afternoon			
	— — —	2 9 8	16 15	Morning	Sun's upper limb	55,32	
	— — —	9 0 28	— — —	Afternoon			
	9 38	2 16 58	24 8	Morning	Sun's lower limb		
	0 0	9 0 0	— — —	Afternoon			
	— — —	3 24 23:	34 14:	Morning	Sun's upper limb	50,54	hazy
	— — —	45 8:	35 15:	Afternoon			
	25 6:	3 35 12:	45 48:	Morning	Sun's lower limb		
	44 23:	7 34 16:	23 38:	Afternoon			
18	17 16	0 23 28	29 30	Morning	Sun's upper limb	64,36	
	17 13	11 11 3	4 58	Afternoon			
	23 50	0 30 6	36 8	Morning	Sun's lower limb		
	10 40	11 4 2	58 20	Afternoon			
	— — —	0 39 11	45 17	Morning	Sun's upper limb	63,20	
	— — —	0 0 0	49 16::	Afternoon			
	39 37	0 45 51	— — —	Morning	Sun's lower limb		
	— — —	10 48 43	— — —	Afternoon			
	— — —	2 22 1	29 5	Morning	Sun's upper limb	55,25	
	— — —	9 12 22	5 24	Afternoon			
	22 30	2 29 49	— — —	Morning	Sun's lower limb		
	11 54	9 0 0	— — —	Afternoon			
	— — —	3 32 20	41 56	Morning	Sun's upper limb	51,3	
	— — —	8 2 7	52 26	Afternoon			
	32 58	3 42 50	53 6	Morning	Sun's lower limb		
	1 27	7 51 35	41 20	Afternoon			

2

From

From the above correſponding Altitudes the going of the Clock is determined.

		Apparent noon per clock, per equal altitudes.	Clock too ſlow for ſidereal time.	Rate of clock with reſpect to ſidereal time.	
		h ′ ″	′ ″	″	
May	6	2 53 31,0	1 1,0	+4,6	
	8	3 1 25,2	0 52,3	+1,2	
	15	3 28 59,2	0 43,2	—1,3	
	20	3 48 43,9	0 49,6		Clock ſtopped.
♄ June	3	4 45 5,1	1 13,6	—0,5	
	9	5 9 46,4	1 16,6	+1,3	
	10	5 13 56,9	1 15,3	+2,1	
	11	5 18 7,3	1 13,2	—0,2	
	15	5 34 42,5	1 14,0	+0,07	
	18	5 47 10,3	1 14,2		
			Mean rate of clock	+0,91	

TRANSITS taken with a Tranſit Inſtrument over the Meridian, on the Iſland of Magge[r] or the North Cape of Europe.

1769

♄ May 20 | $3^h\ 49'\ 30'',7$ tranſit of Sun's center at tranſit inſtrument, it being adjuſted to meridian mark, which was put nearly in the meridian. ——— And $3^h\ 48'\ 43'$ apparent noon per equal altitudes. By this it appears, the meridian mark is w[eſt] of the true meridian.

21 | Shifted the meridian mark nearer to the true meridian.

		1 Wire. ′ ″	2 Wire. ′ ″	3 Wire. h ′ ″	4 Wire. ′ ″	5 Wire. ′ ″	
♄ June	3		43 11½	4 44 1½			☉ 1 L.
				46 17	47 7½		☉ 2 L.
♀	9			5 10 58,6			☉ 2 L. paſſed the mer[idian]
		11⅝	27 8+	18 28 5—	29 2—	58—	α Lyræ.
		59—	43⅝	19 38 28+	13⅜	58	α Aquilæ.
♄	10	47 8	47 54	10 48 39	49 24½		☽ 1 L.
				12 45 14			Polaris S. P.
		32 v	19—	14 4 6—	53½		Arcturus.
		14 v	11—	18 28 7+	4+	0⅝	α Lyræ.
☉	11	15 24—	16 12+	5 17 0½			☉ 1 L.
				19 18½	20 7⅝	20 55½	☉ 2 L.
♃	15		32 46½	5 33 34½			☉ 1 L.
				35 53+	36 41½		☉ 2 L.
♀	16	17 52+	18 39—	4 19 26—	20 13+		♀ 2 L.
♄	17	40 18	41 6+	5 41 54+			☉ 1 L.
				44 15½:	45 2½	45 50⅝	☉ 2 L.
		33+	3 20½	14 4 7½	4 55		Arcturus.
		42 22+	43 11—	17 43 59+			☉ 1 L.
				46 17½	47 5⅝	47 54+	☉ 2 L.
		26 14	27 11	18 28 7½	4½		α Lyræ.
			17 31—	4 10 17½	19 4⅜		♀ 2 L.
☉	18	44 28—:		5 46 24½			☉ 1 L. } flying clouds.
			47 35—:	48 23½	49 12½		☉ 2 L. } flying clouds.
				12 45 30:			Polaris S. P.

Tranſi[ts]

TRANSITS compared with equal Altitudes, for finding the Error of the Meridian Mark.

	Transits of Sun at transit instrument.	Noon per equal altitudes.	Differences.
	h ′ ″	h ′ ″	″
June 3	4 45 9,3	4 45 5,1	4,2 W.
11	5 18 9,9	5 18 7,3	2,6 W.
15	5 34 43,9	5 34 42,5	1,4 W.
18	5 47 13,0	5 47 10,3	2,7 W.
	Mean difference		2,7 West.

Hence it appears, that the Sun passed at the transit instrument too late 2″,7 by a mean of these observations, which shews the mark was west of the true meridian; which gives the azimuth of mark = 50″ south westerly.

TRANSIT of VENUS, observed at the NORTH CAPE.

1769 ♄ June 3 At $13^h\ 46'\ 40''$ per clock, or $9^h\ 0'\ 2''$, apparent time, the Sun came out from under a cloud, with Venus on it, about $\frac{1}{4}$th of her diameter; and at $14^h\ 0'\ 41''$ or $9^h\ 14'\ 1''$, apparent time, Venus's outer limb seemed to be in contact with the Sun's limb; but no light of that part of the Sun's limb could be seen, Venus being apparently joined to the Sun's limb by a black ligament, which gradually diminished in breadth; and at $14^h\ 1'\ 36''$, or $9^h\ 14'\ 56''$, the Sun's light broke through it, and Venus and the Sun were to appearance perfect (this was certain to about 10 or 15 seconds of time), and the black ligament contracted itself, so that Venus was considerably within the Sun's limb, suppose $\frac{1}{20}$th of her diameter.

During these observations the air was red and hazy, and the Sun's limb very tremulous, and the spots in the Sun very indistinct, and Venus seemed very ill defined when on the Sun. But a better idea will be formed of the bad appearance of Venus at the internal contact, owing to the very hazy state of the air, from the representation of it, plate XIII.

Here follow some measures I was able to take during the time the clouds were off the Sun.

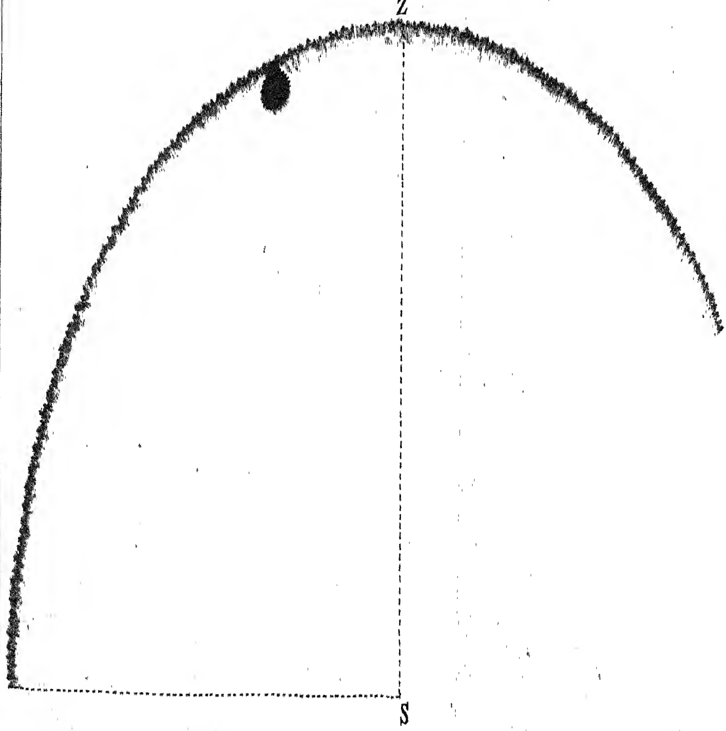
Soon after the internal contact, measures of Venus's diameter.

Time per clock.	App. time.	Measures of microm. in inches, &c.	Measures of microm. in degrees, &c.
		0,1 24	
		0,1 23 +	
		0,1 23 —	
		0,1 22 +	
		0,1 22	
		0,1 22	
		0,1 23 —	
		0,1 24 —	
		0,1 23 —	
		0,1 23	
		0,1 23 —	
		0,1 23 —	
Mean of	= 12	0,1 22,8	= 55″,32

Measures of the Sun's horizontal and vertical diameters, taken immediately after Venus's diameter.

	Horizontal Diameter.		Vertical Diameter.	
	4,9½ 21½		4,8½ 12	
	4,9½ 23		4,8½ 10	
	4,9½ 23		4,8½ 11	
	4,9½ 21½		4,8½ 9	
Mean	4,9½ 22,2	31′ 37″,61	4,8½ 10,5	30′ 50″,73

Difference



J. Mynde sc.

Difference of Declinations of the North Limbs of the Sun and Venus.

Time per clock. h	′	″	App. time. h	′	″	Meafures of microm. in inches, &c.		Meafures of microm. in degrees, &c. ′	″
14	38	37	9	51	50	0,5½	1	3	29,7
	44	30		57	32	0,5½	9	3	35,8
	46	56	10	0	8	0,5½	13	3	38,8
	52	45		5	56	0,5½	23	3	46,4

Difference of Declination of the South Limbs of the Sun and Venus.

14	54	50	10	8	1	4,2½	5	26	58,6
	57	43		10	53	4,2½	0	26	54,8
	59	52		13	2	4,2	20	26	51,0
15	2	0		15	9	4,2	16	26	48,0

Equatoreal Diftances of the Weftern Limbs of the Sun and Venus.

15	8	5	10	21	13	3,1	17	19	50,8
	10	28		23	36	3,1	2	19	39,4
	12	15		25	23	3,0½	20	19	34,1
	13	48		26	56	3,0½	12	19	27,9

Equatoreal Diftances of the Eaftern Limbs of Venus and the Sun.

15	15	50	10	28	57	6,8½	8½	11	49,4
	17	38		30	45	1,8½	20	11	58,1
	19	33		32	40	1,9	8	12	8,0
	22	0		35	6	1,9	19	12	16,4
	24	5		37	11	1,9½	7	12	26,2

Greateft and leaft Diftances of Venus's neareft Limbs from the Sun's Limbs, for finding the neareft Diftance of their Centers.

15	49	3	11	2	4	0,6½	17	4	19,9
	51	22		4	23	4,0	2+	25	21,3
	54	24		7	25	0,7	7	4	31,3
	58	1		11	1	3,9½	10	25	8,4
16	0	40		13	39	3,9½	7	25	6,1
	4	4		17	3	0,7	23	4	43,4
	6	24		19	22	0,7½	5	4	48,8
	9	55		23	53	3,9	14	24	52,5
	16	29		29	26	0,7	12	4	35,1
	21	31		34	27	0,7	8	4	32,1
16	25	55	11	38	51	3,8½	20::	24	38,0::

A Table for reducing the Scale of the micrometer to Degrees, &c.

Inches.	Value. ′	″
1	6	19,95
2	12	39,91
3	18	59,86
4	25	19,81
5	31	39,77

Tenths.	Value. ′	″
1	0	37,99
2	1	15,99
3	1	53,98
4	2	31,98
5	3	9,97
6	3	47,97
7	4	25,97
8	5	3,96
9	5	41,96

Vernier.	Value ″
1	0,76
2	1,52
3	2,28
4	3,04
5	3,8[illegible]
6	4,5[illegible]
7	5,32
8	6,08
9	6,84
10	7,60
11	8,36
12	9,12
13	9,88
14	10,64
15	11,4
16	12,16
17	12,9[illegible]
18	13,68
19	14,44
20	15,2[illegible]
21	15,9[illegible]
22	16,7[illegible]
23	17,48
24	18,24
25	18,99

On examination, the micrometer fcale wanted no adjuftment.

ECLIPSE of the SUN, obferved at the NORTH CAPE.

1769
June 3 At $1^h\ 48'\ 4''$, the clouds clearing away, I faw the Sun, and the Moon had made a fmall impreffion or notch in the Sun's limb; by obferving the increafe of the eclipfe, I fuppofe it began 4, 5, or 6 feconds fooner than I firft faw it, or at $1^h\ 48''\ 0''$ per clock, or $20^h\ 59'\ 19''$ apparent time, nearly.

Diftances of the Cufps.

Time p clock h ′ ″	App. time h ′ ″	Meafures	Reduced ′ ″
2 10 22	21 21 0	3,7 5	23 29,6
13 19	24 34	3,8½ 9	24 29,6
19 1	30 15	4,1½ 6	26 21,3
21 3	32 16	4,2 7	26 41,1
23 50	35 3	4,3½ 8	27 38,8
27 38	38 50	4,4½ 14	28 21,4
31 10	41 21	4,5½ 10	28 56,4
34 56	46 7	4,6 13	29 17,7
36 4	47 14	4,6½ 15:	29 38,2
39 11	50 21	4,7 0	29 45,8

Sun's horizontal Diameter, meafured directly after the Eclipfe ended.

	4,9½ 21½
	4,9½ 20
	4,9½ 20
	4,9½ 22+
	4,9½ 21−
Mean	4,9½ 20,9=31′ 35″,9

Meafures of the lucid Part, near the Middle of the Eclipfe.

2 43 7	21 54 16	1,1 5	7 1,7
44 11	55 20	0,9½ 22	6 17,7
47 50	58 59	0,9 24	6 0,2
49 35	22 0 43	0,9 9	5 48,4
52 14	3 22	1,0½ 11	6 47,3

Diftances of the Cufps.

2 54 55	22 6 2	4,6½ 19	29 41,2
57 1	8 8	4,7 3	29 48,1
59 55	11 2	4,6 14	29 18,4
3 2 24	13 30	4,6 10	29 15,4
4 31	15 37	4,6 7	29 13,1
6 37	17 42	4,5½ 9	28 55,6
8 13	19 18	4,5 20	28 45,0
10 34	21 38	4,4½ 18	28 24,5
14 57	22 26 1	4,3 17	27 26,7

By comparing the end with Mr. Mafkelyne's obfervation at Greenwich, the difference of meridians comes out $1^h\ 44'\ 6''$ of time. = $26°\ 1'\ 30''$ eaft, or difference of longitude of my obfervatory from Greenwich to the eaft. From whence the longitude of the point called the North Cape is $25°\ 49'$ eaft of Greenwich.

Clouds came on, fo that I faw the Sun no more till $3^h\ 38'\ 0''$ per clock, and it broke away very clear, and continued clear to the end, which was at $3^h\ 48'\ 19''$ per clock, or $22^h\ 59'\ 17''$ apparent time. The air being very clear, the end feemed certain to about two feconds.

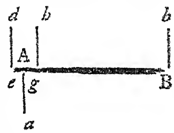
The telefcope ufed was a reflector of 2 feet focus, made by Mr. Dollond; and the magnifying power, applied for the ingrefs of Venus, and the beginning and end of the folar eclipfe, was 100. The magnifying power ufed with the micrometer, was 50.

Adjuftments of the telefcope with the different eye-pieces and little fpeculums, as combined together when ufed. N. B. — fhews that (o) on the vernier, is behind, or to the left hand of the firft divifion on the fcale; and + to the right hand, or before.

Long eye-piece and fhort focus little fpeculum magnifying 100 times $= - 0 + 11\frac{1}{2}$ on vernier.
Long eye-piece and long focus little fpeculum with micrometer on $= - 0 + 19\frac{1}{3}$ on vernier.
The eye-piece with moveable wires, and long focus little fpeculum, and micrometer $\}$ $= + 0 + 4$ on vernier.
Thefe are a mean of 10 or 12 obfervations each.

The Value of the Scale of the Object Glafs Micrometer was found as follows:

A bafe of 120 feet was carefully meafured on level ice (which was covered with frozen fnow about half an inch thick), with two 10 feet fir rods; and the meafure being taken four different times, no one differed from any of the others fo much as $\frac{1}{10}$th of an inch. But as the fir rods might not confift of 10 feet exactly of the fame ftandard from which the micrometer fcale was laid off, I therefore took fix inches between the fine points of a pair of compaffes, from the micrometer fcale; and, by repeating this meafure, found the exact length of the fir rods in meafures of the micrometer fcale; and thence corrected the length of the bafe, found immediately by the rods, and reduced to the ftandard of the micrometer fcale. Perpendicularly over the bafe, at one end, was placed the center of the divided object glafs, and perpendicularly over the bafe, at the other end, a board was placed, having its plane at right angles with the bafe line, white paper pafted on its upper part; and at the height of the center of the object glafs, nearly above the horizon, was a ftrong black line drawn, as $AB = 8$ inches, Ag and Ae being fmall but equal diftances from A; by bringing (ed and gb) to coincide alternately with (Aa) the error of adjuftment of the micrometer fcale was found; and by feparating the glaffes till (Aa) and (Bb) made but one line; from this feparation the value of one extent of the micrometer fcale was found, by making this proportion. As the length of the bafe : is to the length (AB) :: fo is 206265″ (the number of feconds in an arch equal to the radius of a circle) to a fourth number, which will be the feconds of the angle meafured by that opening of the glaffes, which is fhewn by the micrometer fcale; and other angles will be in direct proportion to the refpective meafures whence the table was formed.



ZENITH DISTANCES taken with an Aftronomical Quadrant, of one Foot Radius, made by Mr. Bird, at the NORTH CAPE of EUROPE.

1769	Bar. Inches.	Ther. out °	Ther. in °		Interior arch °	′	″	Exterior arch °	′	″	″	Exterior arch reduced °	′	″
⊙ May 14	29,80	43	40	⊙ U. L. on the merid.	51	59	10	55	1	26	15	51	58	59,2
20	29,71	45	41	⊙ L. L. ditto	51	10	40,0	54	2	12	8	51	10	45,9
♄ June 3	29,93	66	58	⊙ L. L.	48	52	44,0	52	0	18	16	48	52	36,6
				⊙ U. L.	48	20	54,0	51	2	9	0	48	20	49,8
11	29,57	46	42	⊙ L. L.				51	1	11	12	48	7	26,7
				⊙ U. L.	47	35	37,0	50	3	3	0	47	35	40,3
15	29,66	41	40	⊙ U. L.				50	2	5	8	47	22	41,3
				⊙ L. L.	47	54	27,8	51	0	13	14	47	54	33,8
16	29,70	44	39	♀ Center	52	13	36+	55	2	27	17	52	13	27,4
17	29,72	44	42	⊙ L. L.	47	50	28,0	51	0	4	0	47	50	30,5
				⊙ U. L.	47	18	50,0	50	1	28	10	47	18	42,0
	29,71	47	$48\frac{1}{2}$	Arcturus	50	36	8,0	53	3	30	16	50	36	21,2
	29,70	52	$51\frac{1}{2}$	⊙ L. L. } on the north meridian.				91	1	12	24	35	37	41,1
				⊙ U. L. }	85	7	29+	90	3	6	[illegible]	35	7	19,4
	29,70	52	$51\frac{1}{2}$	α Lyræ	32	25	18+	34	2	11		32	25	22,0
	29,18	52	$54\frac{1}{2}$	♀ Center	52	27	0,0	55	3	25		52	26	55,4

From

From the foregoing Zenith Distances of the Sun, and of Arcturus, and α Lyræ, the Latitude of the Observatory is determined, as follows:

1769		From	Latitude °	′	″
May	14	☉ U. L.	71	0	43,2
	20	☉ L. L.	71	1	0,1
June	3	☉ Center	71	0	43,5
	11	☉ Center	71	0	39,8
	15	☉ Center	71	0	39,7
	17	☉ Center	71	0	40,6
		Arcturus	71	1	0,9
		α Lyræ	71	0	48,6
		Mean - - - -	71	0	47,0

From whence the latitude of the point of land called the North Cape is 71° 10′ north.

By a great many trials with a very good compass, of Dr. Knight's construction, I found the variation to be 6 degrees west; and by a dipping needle, I found, by repeated trials, the dip of the north end of the needle to be 79 degrees.

May 15, at $13^h \frac{3}{4}$ P. M. apparent time, or $1^h 7'$ after high water, by a mean of 7 observations, I found the dip of the horizon of the sea, from the observatory, to be 12′ 18″. Height of the barometer 29,70 inches; thermometer, without, 24°; thermometer, within, 28°. And May 20, at $7^h \frac{3}{4}$ P. M. apparent time, or $7^h 33'$ after high water, from a mean of 8 observations, I found the dip = 12′ 25″,5; barometer 29,70 inches; thermometer, without, 43°, and, within, 40°. Both these observations were made on the N. N. E. point of the true compass. During each of these observations the water was very smooth, and the horizon clear.——I found it was high water, at the full and change of the Moon, at $3^h 44'$ P. M. apparent time, at the Cape; and, by a series of observations, I found the water to rise 8 feet 1 inch, nearly, perpendicular at the spring tides; and at neap tides 6 feet 8 inches, perpendicular; and the tides seemed to follow very regular, as they ought to do when not disturbed by bad weather.——June 8, I found the height of the observatory 140 feet 6 inches above low water mark.

A JOURNAL of the Barometer and two Thermometers, during the Time I was on Shore in the Island of MAGGERŒ, or NORTH CAPE, viz. from the 1st of May to the 21st of June.

		At noon. Bar.	Ther. out	in
1769		Inches	°	°
May	1	29,72	19	24
	2	29,68	26	28
	3	29.90	18	22
	4	29,96	28	31½
	5	29,92	34	22
	6	29,91	58	38
	7	29,74	60	40
	8	29,84	56	39

At

1769		At noon. Bar. Inches	Ther. out °	Ther. in °	At midnight. Bar. Inches	Ther. out °	Ther. in doors °
May	9	29,90	47	36			
	10	29,97	46	39	29,98	34	36
	11	29,97	39	34	29,96	30	32
	12	29,97	30	31	29,97	27	29
	13	30,03	42	36	30,04	34	37
	14	29,80	43	40	29,74	33	37½
	15	29,73	41	37	29,70	28	30
	16	29,66	29	31	29,64	26	28
	17	29,62	51	42	29,60	39	43
	18	29,48	53	45	29,39	38	41
	19	29,34	51	47	29,57	40	40
	20	29,71	45	41½	29,77	37	41
	21	29,51	54½	47	29,46	37	39
	22	29,51	41	38	29,64	36	43
	23	29,77	40	37	29,78	36	36½
	24	29,65	44	41½	29,47	40	40
	25	29,42	52	45	29,71	44½	45
	26	29,77	44	45	29,78	40	42
	27	29,76	48	44	29,73	36	38
	28	29,70	44	36½	29,66	34	35
	29	29,47	38	33	29,51	38	40½
	30	29,61	50	43	29,61	52	50
	31	29,61	52	48	29,70	42	37
June	1	29,67	41	38	29,72	38	40
	2	29,92	54	50	29,90	48	52
	3	29,93	66	58	29,78	36	36½
	4	29,79	43	43	29,70	46	40
	5	29,77	61	55	29,69	47	49
	6	29,51	47	43	29,50	43	43
	7	29,83	43	47	29,90	42	44
	8	29,89	47	45	29,90	41	40
	9	29,84	49	47½	29,74	40	42
	10	29,67	48	42	29,66	41	43
	11	29,57	46	42	29,58	33	38
	12	29,48	38	35	29,51	31½	33
	13	29,56	38	34	29,52	35	34
	14	29,53	39	36	29,57	35	35½
	15	29,66	41	40	29,64	34	35
	16	29,51	38	38	29,62	38	37½
	17	29,72	44	42	29,70	52	51½
	18	29,18	52	54½	29,52	44	46
	19	29,42	37	39	29,38	36	38
	20	29,67	36	37	29,49	32	34
	21	29,54	36	38	29,60	31½	33
	22	29,46	35	37			

The thermometers were kept, the one in the obfervatory, and the other without, in the open air; but always in the fhade; and were always obferved at noon and midnight, after the 10th of May; but before, only at noon.

The INSTRUMENTS ufed at the NORTH CAPE, by Mr. BAYLY, were as follows:

A quadrant of 1 foot radius, and two thermometers, made by Mr. Bird.

A 2 feet reflector, with an achromatic object glafs micrometer, by Mr. Dollond.

A tranfit inftrument, of 4 feet, made by Mr. Bird; with an achromatic object glafs, by Mr. Dollond.

A barometer, by Mr. Ramfden.

An aftronomical clock with a gridiron pendulum, a journeyman clock, and an alarum clock, by Mr. Shelton.

A dipping needle, belonging to the Royal Obfervatory, made by Mr. Graham.

A like fet, exclufive of a dipping needle, was ufed by Mr. Dixon, at the ifland of Hammerfoft.

N. B. The adjoining chart, and views of the fea-coaft and iflands, near the North Cape of Europe, Tab. XIV. were drawn from the joint obfervations of Meffieurs Dixon and Bayly.

XXXVII. *An Account of an Obſervation of the Tranſit of* Venus, *made at* Iſle Coudre *near* Quebec. *In a Letter to the Reverend* Nevil Maſkelyne, *Aſtronomer Royal, from Mr.* Thomas Wright, *Deputy Surveyor of the Northern Diſtrict of* America.

Quebec, June 15, 1769.

SIR,

Read Nov. 16, 1769.

I WAS prevented landing at the bay of Gaſpée, as I purpoſed (by blowing, thick weather); but, however, I had the good fortune to reach the iſland of Coudre, where I landed, with all my apparatus, the 30th of May; and took up my abode at a houſe well ſituated, in every reſpect, for my purpoſe. The next morning I had a carpenter, who fixed my clock, very firm and perpendicular, againſt a beam of the houſe. I immediately ſet it a-going by my watch, which had not been ſet to true time for almoſt a fortnight; but, however, I doubt not but that the following obſervations of correſponding altitudes will ſhew exactly the time, as

alſo the regular rate of going of the clock, which I did not venture to adjuſt, my time being ſhort.

As it is likely I may ſtay here ſome time, and all next winter, I ſhall endeavour to make ſuch obſervations as may be uſeful in further ſettling the longitude here.

Captain Holland obſerved the external contact, but not the internal, being prevented by clouds. He has ſent them to you by this opportunity.

I am,

SIR,

Your moſt obedient,

humble ſervant,

Tho. Wright.

Corre-

Correſponding double altitudes of the Sun's lower limb, taken with a braſs ſextant, by reflection, from a ſaucer of oil, ſo placed as not to be the leaſt diſturbed with wind.

Thurſday, June 1, on the north-weſt ſide of the iſland of Coudre, in latitude 47° 16′ 30″, determined by ſeveral obſervations of two altitudes, with the interval of time ſhewn by the time-piece.

Morning, June 1. h ′ ″	Dou. alt. ° ′	Afternoon. h ′ ″	Compared ſeparately give h ′ ″
At 8 29 45	75 38	At 4 8 54	12 19 19
8 32 52	76 42	4 5 37	12 19 14
8 35 50	77 43	4 2 39	12 19 14
8 38 31	78 38	4 0 0	12 19 15
8 40 53	79 26	3 57 24	12 19 08
8 43 34	80 20	3 55 0	12 19 17
8 36 54 Mean		4 1 36 Mean	12 19 15 Mean
16 01 36		Add 12	
7 24 42 Interval		16 01 36	

3 42 21 Half Interval
8 36 54 Time in the morn.

12 19 15
—00 00 06 Equat. of correſponding alt.

12 19 09 Time ſhewn by clock at apparent noon
+2 35 Equation of time—from apparent noon

12 21 44 Clock too faſt for mean time

 Friday,

Friday, June 2.

	Morning. h ′ ″	Dou. alt. ☉ low. limb. ° ′	Afternoon. h ′ ″	Compared separately. h ′ ″
At	8 54 28	84 19	3 42 20	12 18 24
	8 56 40	85 00	3 40 12	12 18 26
	8 58 50	85 45	3 38 02	12 18 26
	9 2 34	87 00	3 34 13	12 18 24
	9 4 2	87 27	3 32 45	12 18 24

8 59 19	Mean	3 37 30 Mean
15 37 30		12
6 38 11	Interval	15 37 30
3 19 5½	Half Interval	
8 59 19		
12 18 24½		
—4	Equat. of corresponding altitudes.	
12 18 20½	Clock too fast for apparent time	
+2 26	Equation of time—from apparent	
12 20 46½	Clock too fast for mean time	
21 44	Clock too fast at noon of June 1	
0 57½	Clock has lost in 24 hours	

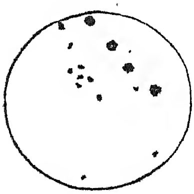
Saturday, June 3, the morning cloudy, no altitudes taken.

h ′ ″	
At 2 49 22	by the clock, I happened to take my eye off from the very point where I afterwards found the external contact happened, imagining I saw it something more to westward; but, finding my mistake, I returned to the former point, where I found Venus had made a very small
2 50 25	impression at $2^h\ 50'\ 25''$, as is set down in the margin.
3 07 48	time when Venus appeared compleatly round to the eye, and to appearance rather detached, and joined by a small dark thread or ligament, which prevented the rays of light from appearing.
3 08 19	time when the rays of light just appeared, at the internal contact.

The

The following is the above times, as ſhewn by the clock, reduced to apparent time, by allowing a proportion of ·57 ſeconds, its regular loſing in 24 hours; as appears by the preceding and the following correſponding altitudes.

h	'	"	'	"	h	'	"	
2	49	22 ——	17	32 =	2	31	50	apparent time of the 1ſt obſervation.
2	50	25 ——	17	32 =	2	32	53	apparent time of the 2d obſervation.
3	7	48 ——	17	31 =	2	50	17	ap. time of 1ſt obſ. of internal contact.
3	8	19 ——	17	31 =	2	50	48	ap. time of 2d obſ. of internal contact.



The appearance of Venus at the internal contact, when joined by a ſmall thread to the Sun's limb; as alſo the ſpots of the Sun, as obſerved at the time of the tranſit, and two days before.

By means of two oblong ſmoaked glaſſes with different ſhades, made to ſlide in a groove fixed to my teleſcope, the phænomenon appeared very diſtinct and pleaſing to the eye, notwithſtanding the weather was a little hazy, and very much ſo, near the horizon. The thermometer ſtood at 74 degrees at the time of obſervation, and the weather was remarkably cloſe and ſultry two days before, and quite calm till an hour before the tranſit happened, when it began to blow very freſh. June 4, the weather continued much the ſame, and about $9^h\ 30'$ in the evening, we had a ſhock of an earthquake, which laſted about four ſeconds, and alarmed all the inhabitants of the iſland.

The weather, at the time of the tranſit, was not clear enough to obſerve the leaſt appearance of an atmoſphere round the planet, ſuppoſing there really had been one.

Saturday,

Saturday, June 3, correſponding double altitudes of the Sun's lower limb for midnight, taken in a ſaucer of oil.

	June 3, Afternoon.			Altitude.		Morn. June 4.			Compared ſeparately.		
	h	'	"	°	'	h	'	"	h	'	"
At	4	4	25	76	58	8	29	41	12	17	03
	4	6	43	76	10	8	27	21	12	17	02
	4	8	34	75	33	8	25	26	12	17	0
	4	10	42	74	50	8	23	20	12	17	01
	4	12	52	74	08	8	21	11	12	17	02

h	'	"		h	'	"	
4	8	39	Mean	8	25	24	Mean
20	25	24		12			
16	16	45	Interval	20	25	24	
8	08	22½	Half Interval				
4	08	39					
12	17	01½	Time of midnight as ſhewn by clock				
		+9	Equat. of correſponding altitudes				
12	17	10½	Clock too faſt for apparent time of midnight				
	+2	11	Equation of time—from apparent				
12	19	21½	Clock too faſt for mean time				
	20	47	Clock too faſt, June 2, at noon				
	1	25½	Clock has loſt in 36 hours				
		57½	Clock loſt in 24 hours by the preceding obſervations				
		28½	Clock loſt in 12 hours by the preſent obſervation, which is very near at the ſame rate.				

Double altitudes, taken with a ſextant, in a ſaucer of oil, for finding the lat. of the place of obſervation.

	June 4, morn.			Alt. ☉ l. limb.		Afternoon.			Doub. alt. ☉ l. limb.		
	h	'	"	°	'	h	'	"	°	'	
At	10	34	7	115	12	2	1	50	114	36	There is 3' to be ſubtracted from the half ∠ for the errors of quadrant.
	10	36	44	116	00	2	4	32	113	46	
	10	37	40	116	13	2	6	24	113	20	
	10	39	26	116	38	2	8	05	112	50	

By

By the firſt of the above obſervations with a ſuppoſed lat. = 47° 15′, being the reſult of a former obſervation, and the Sun's declination (corrected for the longitude) = 22° 31′ 51″ N. and half the elapſed ✕ = 1^h 43′ 51″½ the latitude will be found = 47° 16′ 51″, N.

By the ſecond obſervation, computed in the like manner, the latitude will be 47° 16′ 41″, N.

The place of obſervation on the iſland of Coudre, by an actual ſurvey, bears from Quebec, N. 41° 30′, E. by the true meridian, diſtance 55 ſtatute miles, = 52 marine; which gives D. latitude = 39′ and Dep. 34′ = 50′ D. longitude = 3′ 20″ of time between Quebec and Coudre.

I have here mentioned every particular relative to the obſervation, and as it really happened, that you might, with greater certainty, correct any errors that may be found therein.

To prove the time aſcertained by correſponding equal altitudes, thoſe altitudes taken within an hour of the tranſit might be worked ſeparately, remembering to ſubtract 3′ from the ſingle altitude for the error of the quadrant.

Remarks by the ASTRONOMER ROYAL.

THE inſtruments made uſe of by Mr. Wright, in the foregoing obſervations, were a 2 feet reflecting teleſcope; a pendulum clock beating half ſeconds; a braſs Hadley's ſextant, of about 15 inches radius, with a magnifying glaſs to read off the obſervations; and a rectangular reſervoir for holding quick-

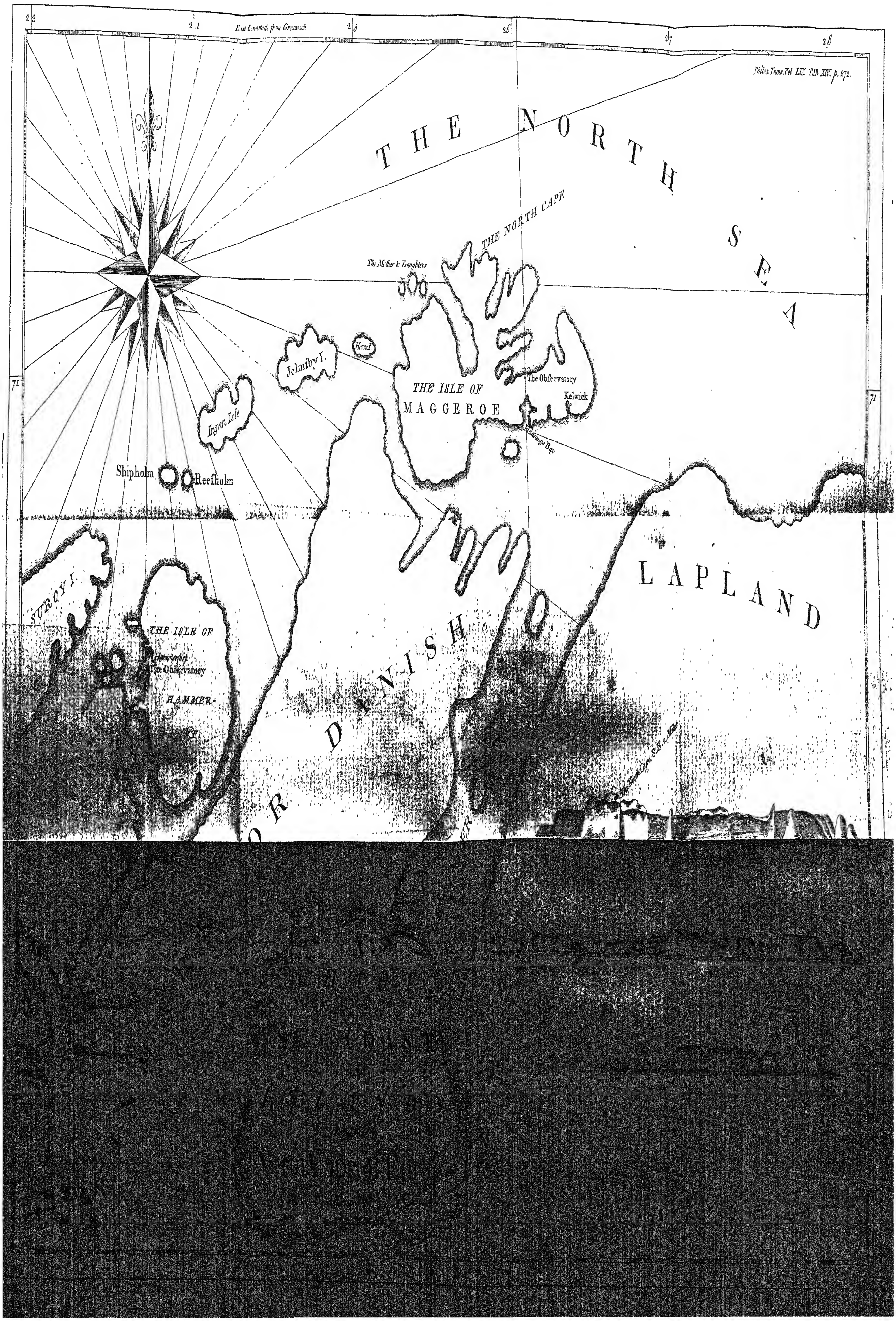
ſilver,

silver, or any other fluid, which is sheltered from the wind by two glass sides inclined to one another, and ground truly plane: this last for taking the Sun's double altitude by reflection with the Hadley's sextant. By a more accurate calculation of the times than Mr. Wright has used, I find the equation of corresponding altitudes, for the noon of June 1 to be $-5'',0$, June 2 $-4'',5$, and June 3 for midnight $+9'',6$. Hence the true time of noon, by the clock, June 1, was $12^h\ 19'\ 10'',0$; June 2, $12^h\ 18'\ 20'',0$; and June 3, midnight, $12^h\ 17'\ 11'',1$; and hence the true time of noon, June 3, should be $12^h\ 17'\ 34'',1$, and the clock is losing $46''$ per day on apparent time. Hence the apparent times of Mr. Wright's 4 observations will come out as follows:

App. time. h ′ ″	
2 31 53	No visible impression made by Venus yet.
2 32 56	Venus had made a small impression.
2 50 19	Venus appeared completely round to the eye, and rather detached, and joined by a ligament.
2 50 50	The rays of light appeared at the internal contact.

Taking Isle Coudre to bear N. 41° 30′ East from Quebec, distant 55 statute miles, as, Mr. Wright says, was found by an actual survey; the distance in geographical miles is 47,65. Therefore the place of observation is 35′ 41″ north of Quebec, and 31′ 34″ east of it, = 46′ 32″ difference of longitude, = 3′ 6″ of time.

XXXVIII. *Ex-*



Philos. Trans. Vol LIX TAB XIV. p. 272.
East Longitude from Greenwich
23
24
25
26
27
28
71
THE NORTH SEA
THE NORTH CAPE
The Mother & Daughters
Jelmsby I.
THE ISLE OF MAGGEROE
The Observatory
Kelwick
Ingen Isle
Shipholm
Reefholm
THE ISLE OF
The Observatory
HAMMER
OR DANISH
LAPLAND

XXXVIII. *Extract of a Letter from Mr.* B. Gooch, *Surgeon, of* Shottisham, *near* Norwich, *to Mr.* Joseph Warner, *F. R. S. and Surgeon to* Guy's *Hospital. Communicated to the Royal Society by Mr.* Warner, November 16, 1769.

Shottisham, September 9, 1769.

DEAR SIR,

Read Nov. 16, 1769.

ACCORDING to your desire, and my promise, I have sent you the wonderful cuticular glove, which I shewed you, when I had the pleasure of your company here. The history of the case, which, I believe, has no precedent, is taken from the gentleman's own relation of it to me in writing, without varying his sense; and confirmed by Mr. Swallow, a surgeon of character at Watton, whose son, I know, was under your tuition. Mr. Swallow attended the patient many times in the fevers which produced these strange phænomena, with whom I took an opportunity of having a particular conversation relative to this matter, that I might be able to speak with the more authenticity. Mr. Swallow has now one of these gloves in his posse[ssion]; the gentleman himself has another, and several he ha[s] given to the curious: yet some have been so sceptic[al]

as to doubt the matter of fact upon ſuch evidence and authority. I wiſh you would get an accurate drawing of the glove; and I ſhall be glad to know, at your leiſure, the ſentiments of the learned, not forgeting your own, upon this extraordinary caſe.

I am, DEAR SIR,

Your ſincere friend,

as well as obliged,

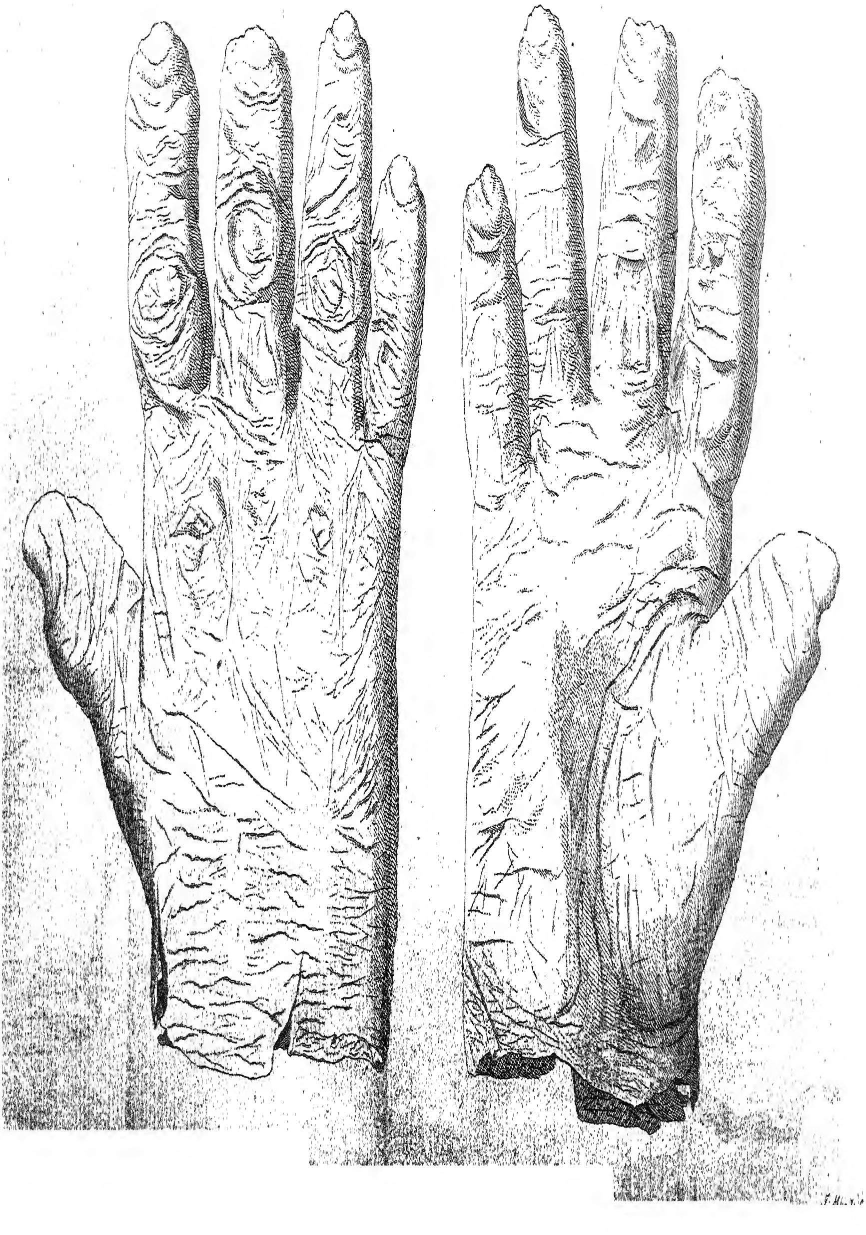
humble ſervant,

B. Gooch.

Hiſtory of the CASE relating to the CUTICULAR GLOVE.

MR. WILLIAM WRIGHT, of Saham Tony in the county of Norfolk, attorney at law, about fifty years of age, rather of a weak and lax conſtitution from his youth, was firſt ſeized about ten years ago with the following ſingular kind of fever. The phyſical gentlemen he at different times conſulted were at a loſs to know what name or character to diſtinguiſh it by. It has returned many times ſince; ſometimes twice in a year, attended with the ſame ſymptoms and circumſtances; but not to ſo great a degree ſince the year 1764 as before; and it has been generally obſerved to come on upon obſtructed perſpiration, in conſequence of catching cold, to which he is very ſubject.

Beſides



Besides the common febrile symptoms upon the invasion of this disease, his skin itches universally, more especially at the joints; and the itching is followed by many little red spots, with a small degree of swelling: soon after his fingers become very stiff, hard, and painful at their ends, and at the roots of his nails. In 24 hours, or thereabouts, the cuticle begins to separate from the *cutis*, and, in ten or twelve days, this separation is general from head to foot; when he has many times turned the cuticle off from the wrists to the fingers ends, completely like gloves; and in the same manner also to the ends of his toes; after which his nails shoot gradually from their roots, at first attended with exquisite pain, which abates as the separation of the cuticle advances; and the nails are generally thrown off by new ones in about six months.

The cuticle rises in the palms of his hands, and soles of his feet, resembling blisters, but has no fluid under it; and when it comes off, it leaves the subjacent skin very sensible for a few days. Sometimes, upon catching cold, before he has been quite free from feverish symptoms, he has had a second separation of the cuticle from the *cutis*, but then it is so thin as to appear only like scurf, which demonstrates the quick renewal of this part.

*** The figure of one of these gloves is seen in TAB. XIV.

XXXIX. *Observatio ingressus* Veneris *in* Solem 3 *die* Junii, 1769, *habita* Gryphiswaldiæ, *ab* Andr. Mayer, *Prof. Reg. Communicated by Mr.* John Ellicot, *F. R. S.*

Read Nov. 16, 1769.

PER omnem diem cœlum ita nubibus tectum erat, ut fere nulla hujus rarissimi phænomeni spectandi spes superesset, donec circa horam septimam vespertinam in ea cœli parte, quam Sol occupaverat, nubes primum rarescere cœperunt; atque post breve temporis spatium liberum Solis conspectum concesserunt. Non tamen sudum omnino aërem, sed vaporibus adhuc repletum fuisse, undabundus Solis margo satis superque docebat. Usus sum tubo Dollondiano, egregiæ virtutis, septem pedes longo, quem postquam in eam marginis Solaris partem direxeram, a Venere in ingressu suo juxta calculum hujus rei gratia institutum contingendam, octava hora 4′ 35″ temporis veri, eodem fere temporis momento, quo etiam Prof. et observat. Reg. clariss. Dn. *Röhl* per tub. refr. 16½ ped. Solem intuenti Venus ingredi visa erat, apparuit mihi unda in margine Solis nonnihil major atque nigricantior reliquis, quam discum Veneris fuisse paulo post convictus reddebar. Accuratiorem hujus ingressus observationem negabat omnino athmosphæræ nostræ conditio.

ditio. Inter continuas et magnas undarum reciprocationes Venus ulterius progressa, difformem valde et mutabilem semper præbebat faciem: ultra dimidium ingressæ diameter horizontalis tribus fere vicibus superabat verticalem, lumenque Solare intra margines Veneris et Solis contentum magis flavescens reliquo imo subrufilum mihi videbatur. Ipsa Veneris facies, distorta nimium, irregularis, atque continuo obnoxia flexui, per reliquum observationis tempus talis erat, qualem figura adjecta exhibet. Paulo ante contactum interiorem, fascia quasi margini Solis alligata visa est, quæ subito soluta $8^h\ 22'\ 44''$ temp. ver. docebat ingressum fuisse factum, cujus mora secundum hanc observationem, erat $18'\ 9''$. Nulla nec satellitis Veneris, nec ejus athmosphæræ vestigia, quæ 1761 nobis tam distincte apparuêre, nisi huc referre liceat, undæ incitatiorem motum in ingressu disci Veneris, atque luminis intra margines Solis et Veneris a reliquo discrepantiam. Nullus in tanta titubatione phænomeni micrometri usus erat, aut filorum, hunc in finem tubis applicatorum. Observatio quidem in suburbii tugurio habita est, ab observatorio ad 2000 passus remoto. Non solum ad penduli motum ex observationibus alt. Solis corresp. correctum tempora notata sunt, sed et beneficio soni ex sclopetis emissi reducta ad motum excellentissimi penduli Ellicotiani in observatorio suspensi.

Puncta designant loca, in quibus lumen Solis subrufilum videbatur.

XL. *Obser-*

Read November 16, 1769.

XL. *Observation of a Solar Eclipse the 4th of* June, 1769, *at the* Observatory *at* Austhorpe, *near* Leeds, *in the County of* York. *By* J. Smeaton, *F. R. S.*

	h	′	″
BEGINNING by mean time, A. M.	6	33	1
Middle - - - - - - - -	7	26	38
End - - - - - - - - - - -	8	20	16
Total duration - - - - - - - -	1	47	15
Digits eclipsed - - - - - - - -	6	46	

N. B. The beginning and end of the eclipse were observed by an excellent $3\frac{1}{2}$ feet treble object-glass telescope, constructed by Dollond, with the smallest magnifier, which enlarged the diameter somewhat above 80 times. As there is no defect in quantity of light from the Sun, the object glass was contracted by an aperture to $2\frac{1}{2}$ inches, and the object was perfectly sharp and distinct.

The quantity was taken by a parallel wire micrometer, upon an equatoreal apparatus, which rendered it very commodious for the purpose; by which the part of the Sun's diameter, remaining uneclipsed, measured at right angles to a line joining the horns, was 889 such parts as the Sun's diameter, taken the same

ſame day at 1½ in the afternoon, meaſured between two parallels of declination, 2041.

The latitude I have not yet got ſo correctly as I expect to do; but I do not at preſent know, whether it exceeds or falls ſhort of 53° 48′. The ſuppoſed longitude is 6′ of time weſt of Greenwich; this is deduced from its poſition with Wakefield, whoſe longitude is ſet down in Maſkelyne's Britiſh Mariner's Guide, as determined from an obſervation of the tranſit of Venus, 1761.

The exact knowledge at what point of the Sun's circumference to look for the beginning (which was communicated to me by Mr. Maſkelyne), I found of great uſe; inſomuch that, I believe, I ſaw the firſt diſcernible impreſſion; I have, however, allowed 2″ for the time elapſed between the firſt perception, and the being ſure it was the approach of the Moon that affected that part of the Sun's limb; and which latter only could be noted by the clock. The firſt approach did not, however, affect the Sun's circumference by any thing like a penumbra or ſhade; but began by ſome aſperities of the Moon's limb, ſeeming to thruſt themſelves into that of the Sun; and which appeared before any continued part of the Sun's circumference was cut off; or, perhaps, it might be occaſioned by the firſt approach of the Moon's limb, diſturbing the little protuberances upon the Sun's circumference, occaſioned by the undulation of the air, and which, when rendered exceedingly diſtinct, appeared almoſt like the teeth of a fine ſaw. This whole appearance, to a teleſcope leſs diſtinct, would probably look like a penumbra or ſhadow.

 Some

Some time before the great ſpot was immerged, there appeared two parts of the Moon's circumference more protuberant than the reſt, near the right hand horn; which ſo remarkably interrupted the regularity of the curve, that it was taken notice of by all about me; and which, doubtleſs, was occaſioned by two mountains upon the Moon's ſurface, remarkably higher than the reſt; and I doubt not but the ſame thing will have occurred to other obſervers.

*** Mr. Smeaton was prevented, by clouds, from obſerving the entrance of Venus upon the Sun, the evening before.

END OF PART I.

PHILOSOPHICAL TRANSACTIONS.

VOLUME LIX. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. LIX. PART II. For the Year 1769.

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M.DCC.LXX.

PHILOSOPHICAL TRANSACTIONS.

PART II.

XLI. *Account of the Tranſit of* Venus *over the* Sun's *Diſk, as obſerved at* Norriton, *in the County of* Philadelphia, *and Province of* Pennſylvania, June 3, 1769. *By* William Smith, *D. D. Provoſt of the College of* Philadelphia; John Lukens, *Eſquire, Surveyor-General of* Pennſylvania; David Rittenhouſe, *A. M. of* Norriton; *and* John Sellers, *Eſquire, one of the Repreſentatives in Aſſembly for* Cheſter *County; the Committee appointed for that Obſervation, by the* American Philoſophical Society, *held at* Philadelphia, *for promoting uſeful Knowledge. Communicated to the ſaid* Society, *in Behalf, and by Direction, of the Committee, by Doctor* Smith; *and to the* Royal Society *of* London, *by* Nevil Maſkeline, *B. D. Aſtronomer Royal.*

GENTLEMEN,

Read Nov. 23, 1769.

AMONG the various public-ſpirited deſigns, that have engaged the attention of this Society ſince its firſt inſtitution, none

does

does them more honour than their early reſolution to appoint Committees, from their own members, to make as many obſervations, in different places, of that rare phænomenon, the tranſit of Venus over the Sun's diſk, as they had any probability of being able to defray the expence of, either from their own funds, or the public aſſiſtance they expected.

As the members of the Committee above mentioned live at ſome diſtance from each other, I am therefore now, by their direction, and in their behalf, to collect, and lay before you, the whole of the Norriton obſervations; diſtinguiſhing however, ſo far as may be neceſſary, the part of each obſerver; and going back to the firſt preparations. For I am perſuaded that the dependance which the learned world will place on any particular tranſit account, will be altogether in proportion to the previous and ſubſequent care, which they find hath been taken, in a ſeries of accurate and well-conducted obſervations, for regulating the time-pieces, and aſcertaing the latitude and longitude of the place of obſervation, &c. And I am the more deſirous to be particular in theſe points, in order to do juſtice to Mr. Rittenhouſe, one of our Committee; to whoſe extraordinary ſkill and diligence is owing whatever advantage may be derived in theſe reſpects to our obſervation of the tranſit itſelf. It is further conceived, that the learned and curious will be deſirous to have not only the work relative to each particular tranſit account, but the materials alſo, that they may have an opportunity to examine and conclude for themſelves. And it is a pleaſure to us that we are able to communicate ſo complete a ſet of obſervations in every material article.

The

The great diſcouragement which the different Committees laboured under, at their firſt appointment by this Society, was the want of proper apparatus, eſpecially good teleſcopes. The generoſity of our Provincial Aſſembly ſoon removed a very conſiderable part of this diſcouragement, not only by their vote to purchaſe one of the beſt reflecting teleſcopes, with Dollond's micrometer, but likewiſe by their ſubſequent benefaction of one hundred pounds, for erecting obſervatories, and defraying other incidental expences. It was foreſeen that, on the arrival of this teleſcope *, added to other private ones that could be procured in this city, and the fitting up the inſtruments belonging to the honourable Proprietaries, viz. the large aſtronomical ſector, and the equal altitude and tranſit inſtrument, nothing would be wanting for the city obſervatory, erected in the State-houſe Square, but a good time-piece, which was eaſily to be procured.

We remained, however, ſtill at a loſs how to furniſh the Norriton obſervatory. But this difficulty gradually vaniſhed. Early in September, 1768, ſoon after the nomination of the Committees, I received a letter from that worthy and honourable gentleman, Thomas Penn, Eſquire, one of the Proprietors of this province, expreſſing his deſire that we would exert ourſelves in obſerving the tranſit, for which our ſituation was ſo favourable; and incloſing ſome copies of Mr. Maſkelyne's directions for that purpoſe.

* This teleſcope came to hand in April, 1769. It was made by Nairne, 4½ feet, and its greateſt magnifying power 400 times; with an excellent micrometer.

This gave an opportunity, which I embraced immediately, to inform Mr. Penn, that this Society, before the receipt of his letter, had appointed two * Committees for the business he proposed; that the Assembly had generously ordered one hundred pounds, sterling, to purchase one telescope, as above set forth; but that we should be at a great loss for a telescope and micrometer, of the like construction, for the Norriton observatory; requesting him to order a reflector of 2½ feet, with Dollond's micrometer, to be got ready in London; which I was in hopes I should prevail on our College to pay for, and take for their own use, as soon as the corporation should have an opportunity of meeting. It was not long before I had the pleasure to hear that Mr. Penn had ordered such a telescope. which came to hand in due time, with a most obliging letter, expressing his satisfaction in the spirit shewn at Philadelphia for observing this curious phænomenon, and concluding as follows:

"I have sent by Captain Sparks a reflecting te-
"lescope, with Dollond's micrometer, exact to your
"request, which I hope will come safe to hand.
"After making your observation with it, I desire
"you will present it, in my name, to the College.
"Messieurs Mason and Dixon tell me, they never
"used a better than that which I formerly sent to the
"Library Company of Philadelphia, with which a
"good observation † may be made, although it has no
"micrometer."——

* A third Committee was afterwards appointed, to observe at or near Cape Henlopen.

† Mr. Owen Biddle, who was appointed by the Society to conduct the observation near Cape Henlopen, had this telescope;

We

We were now enabled to furniſh the Norriton obſervatory, as follows:

1. A $2\frac{1}{2}$ feet Gregorian reflector, with a Dollond's micrometer, made by Nairne, its magnifying powers 55, 95, 135, and 200 times. The gift of the honourable Thomas Penn, Eſquire, to the College of Philadelphia. Uſed by Doctor Smith.
2. A refractor of 42 feet, its magnifying power about 140. The glaſſes ſent to the Aſſembly, with the large reflector, from England. Uſed by Mr. Lukens.
3. Mr. Rittenhouſe's refractor, with an object glaſs of 36 feet focus, and a convex eye glaſs of 3 inches, magnifying about 144 times. Uſed by himſelf.
4. An excellent clock; a tranſit teleſcope, nicely moving in the plane of the meridian; and a very accurate equal altitude inſtrument, ſupported, in the obſervatory, on a ſtone pedeſtal. Theſe three articles were alſo Mr. Rittenhouſe's private property, and made by himſelf, whereof ſome mention is made below.
5. An aſtronomical quadrant, $2\frac{1}{2}$ feet radius, made by Siſſon; the property of the proprietors of Eaſt New Jerſey; under the care of the right honourable William Earl of Stirling, ſurveyor-general of that province, who kindly permitted us the uſe of it.

nothing being deſired here but the contacts, and their exact time, which he obtained to great ſatisfaction, as by his report may appear.

As

As Mr. Rittenhouſe's dwelling at Norriton is 20 miles N. W. of Philadelphia, our other engagements did not permit Mr. Lukens, or myſelf, to pay much attention to the neceſſary preparations; but we knew that we had entruſted them to a gentleman on the ſpot, who had, joined to a complete ſkill in mechanics, ſo extenſive an aſtronomical and mathematical knowledge, that the conſtruction, uſe, and management of all the neceſſary apparatus are perfectly familiar to him. The dull and rainy weather prevented our ſetting out for his houſe till Thurſday, June 1; and we found, on our arrival there, every preparation ſo forward, that we had little to do but to examine and adjuſt our reſpective teleſcopes to diſtinct viſion. Mr. Rittenhouſe had compleated his obſervatory, fitted up the different inſtruments, and made a great number of obſervations, for fixing the latitude and longitude of the obſervatory, and aſcertaining the going of his time-piece. The laudable pains he has taken in theſe material articles will beſt appear from the work itſelf, which I received from him a few days ago, with the following very obliging letter; giving me a liberty which his own accuracy, care, and abilities leave me no room to exerciſe; and therefore what follows is entirely as he drew it up, viz.

"To the Reverend Doctor SMITH.

"Dear Sir,

"The encloſed is the beſt account I can give of "the contacts, as I obſerved them, and of what I "ſaw during the interval between them. I ſhould "be glad you would contract them (and the other

"papers)

"papers) into a ſmaller compaſs, as I would have
"done myſelf, if I had known how. I beg you
"would not copy any thing merely becauſe I have
"written it, but leave out what you think ſuper-
"fluous.

"I am, with great eſteem and affection, Yours,

Norriton, July 18, 1769. "DAVID RITTENHOUSE."

Mr. Rittenhouſe's account of the obſervations made by him, at Norriton, before and after the tranſit of Venus, June 3, 1769; for determining the longitude and latitude of his obſervatory, regulating his clock, &c.

"Early in November, 1768, I began to erect an
"obſervatory, agreeable to the reſolutions of the
"American Philoſophical Society; but, through
"various diſappointments from workmen and wea-
"ther, could not complete it till the middle of
"April, 1769. I had for ſome time expected the
"uſe of an equal altitude inſtrument from Philadel-
"phia; but finding I could not depend on having
"it, I fell to work and made one, of as ſimple a
"conſtruction as I could contrive. It has a good
"teleſcope of $3\frac{1}{2}$ feet focal length, with two hori-
"zontal and one vertical wire, in the focus, and is
"very eaſily adjuſted to a plummet wire, 4 feet in
"length, by two ſcrews; one of which moves it in
"a north and ſouth, the other in an eaſt and weſt
"direction. The 20th of March this inſtrument

"

" was finiſhed, and put up out of doors, the obſer-
" ſervatory being not yet ready.

" I had, however, for ſome weeks before this, " with my 36 feet refractor, obſerved eclipſes of " Jupiter's firſt ſatellite, in ſuch a manner, that " though my equal altitude inſtrument was not fi- " niſhed, and conſequently I could not ſet my time- " piece to apparent noon, I ſhould, notwithſtand- " ing, be able to tell the true time of thoſe eclipſes " afterwards, when the inſtrument ſhould be fi- " niſhed. For this purpoſe, I obſerved, almoſt every " fair evening, the time by the clock, when the " bright ſtar in Orion diſappeared behind a fixed " obſtacle, by applying my eye to a ſmall ſight- " hole, made through a piece of braſs, faſtened to a " ſtrong poſt. The obſervations were as follows:—

1769.	Star diſappeared per clock.	Immerſions of 1ſt ſatellite, per clock.		Equal altitudes obſerved.			Hence the Sun on meridian per clock.
Day.	h ′ ″	February. Day.	h ′ ″	March. Day.	h ′ ″	h ′ ″	h ′ ″
Febr. 15	9 26 39	16	14 24 58	19	8 58 52	2 56 52	11 57 37
22	8 58 52	23	16 17 41	19	9 2 13	2 53 32	
24	8 50 57						
March 3	8 23 21	Hence from the equal altitudes in column 3; the apparent times of the two above immerſions are		20	8 56 40	2 58 26	11 57 18
12	7 48 26			20	8 59 59	2 55 7	
14	7 40 41						
17	7 29 4						
20	7 17 16	Feb.	h ′ ″				
21	7 13 21	16	14 21 10				
28	6 45 44	23	16 15 1				

" From March 20 to May 20, the clock was al- " tered ſeveral times, once taken down to be cleaned, " removed back to the obſervatory, and regulated " anew. Care, however, was taken to obſerve equal

" altitudes

" altitudes of the Sun on the day preceding and fol-
" lowing any visible eclipse of the 1st satellite.
" The eclipses and other observations, during this
" period, were the following:——

Equal altitudes of the Sun.		Sun on meridian per clock.	Observ'd immersions of 1st satellite.
April 3, 1769.			April 3.
A. M.	P. M.		Immersion
h ′ ″	h ′ ″	h ′ ″	h ′ ″
8 5 22	4 1 56		
8 8 16	3 59 2	12 3 25	14 52 40
4th.			
8 3 43	4 3 3		
8 6 38	4 0 10	12 3 9	
10th.			10th.
8 32 8	cloudy.		Day-light
8 35 6			16 46 20
8 36 31			
11th.			
8 30 22	3 30 43		
8 35 6	3 27 47	12 0 20	
8 36 31	3 26 22		
12th.			12th.
8 28 55	cloudy.		
8 31 51			11 14 38
8 33 16			
14th.			
8 25 42	3 33 56		
Cloud	3 31 1	11 59 38	
8 30 2	2 29 37		

Equal altitudes of the Sun.		Sun on meridian per clock.	Observed eclipses of 1st satellite.
May 4th.			
A. M.	P. M.		
h ′ ″	h ′ ″	h ′ ″	h ′ ″
8 5 15	3 44 6		
8 8 3	3 41 18	11 54 32	
8 9 23	3 39 48		
5th.			May 5th.
8 4 11	3 44 51		Immersion
8 6 59	3 42 4	11 54 22	11 23 45
8 8 19	3 40 42		
6th.			
8 3 8	3 45 37		
8 5 54	3 42 51	11 54 14	
8 7 15			
11th.			
8 34 51	3 17 12		
8 36 13	3 15 49	11 55 54	
8 37 40	3 14 22		
8 39 3	3 12 59		
15th.			May 14th.
9 12 59	2 39 28		Emersion
	2 38 2	11 56 7	9 58 20
9 15 53	2 36 32		
	2 35 7		

" May 20, in the morning, the clock was set up
" for the last time, pretty near the mean time. It
" had no provision for preventing the irregularities

 " arising

" arifing from heat and cold; nor could I find lei-
" fure to apply any contrivance of this fort. It had
" been made fome time before, to determine the va-
" riations arifing from thofe caufes. The pendulum-
" rod is a flat fteel bar, with a bob weighing about
" twelve pounds, and vibrates in a fmall arch. It
" goes eight days, beats dead feconds, does not
" ftop when wound up, and is kept in motion by a
" weight of five pounds.

" The ill ftate of my health would not permit
" me to fit up at nights, to take equal altitudes
" of the ftars. I was therefore obliged to content
" myfelf with thofe of the Sun only. I had,
" fome time before this, viz. May 12, got a te-
" lefcope fixed in the meridian, on an axis with
" fine fteel points; fo that the hair in its focus
" could move in no other direction than along
" the meridian. I fet up two marks, north and
" fouth, about 60 rods diftant each, to which it
" can readily be adjufted, in a horizontal pofition,
" by a fcrew; as it can likewife by another fcrew in
" a vertical pofition. The two marks were fhifted
" from day to day, until they were found within lefs
" than one fecond of time of the true meridian.

" May 20, I likewife put wires, inftead of hairs,
" in the telefcope of the equal altitude inftrument;
" and the following are the obfervations taken both
" with it, and with the meridian or tranfit telefcope,
" in the order wherein they were made:

Equal

May	Equal altitudes of the Sun.	A. M. h ′ ″	P. M. h ′ ″	Hence the Sun on meridian. h ′ ″	Obſerved eclipſes of Jupiter's ſatellites. h ′ ″	Obſervations with the meridian teleſcope.	h ′ ″	Hence the Sun's center on meridian. h ′ ″
20	☉'s up. limb at 1ſt hair	8 1 30	3 51 28		Emerſions.	☉'s weſtern limb at meridian	11 55 16	
	D° at 2d hair	8 2 52	3 50 8	11 56 23¾				11 56 23½
	☉'s l. limb at 1ſt hair	8 4 15	3 48 45			☉'s eaſtern limb at ditto	11 57 31	
	D° at 2d hair	8 5 36	3 47 24					
21	N. B. As the Sun ſeems to deſcend, being inverted in the teleſcope, his up. limb is ſet down as com-	8 1 1	3 52 11	11 56 30	1ſt Satellite. Em. 11 51 46	☉'s weſtern limb	11 55 23	11 56 30
		Clouds	3 50 50			☉'s eaſtern ditto.	11 57 37	
			3 49 27			♀'s center on merid.	1 18 39	
			3 48 7					
23	ing to the upper hair firſt—though they might as properly be called 1ſt and 2d as upper and lower hairs	8 0 4	3 53 36	11 56 45		☉'s weſtern limb	11 55 39	11 56 46
		8 1 24	3 52 16			Eaſtern ditto	11 57 53	
		8 2 47	3 50 53					
		8 4 8						
24						☉'s eaſtern limb	11 58 0	
						—Paſſage of ☉'s ſemi-diameter	0 1 8	11 56 52
						♀'s center on meridian hair	1 2 4	
25		7 59 15	3 54 57	11 57 1		☉'s weſtern limb	11 55 53	11 57 1
		8 0 35	3 53 38			Eaſtern ditto	11 58 9	
		8 1 58	3 52 15					
		8 3 18	3 50 54					
26		7 58 54	3 55 38			☉'s weſtern limb	11 56 3	11 57 10½
		7	3 54 18			Eaſtern ditto	11 58 18	
		8 1 37	3 52 56					
		8 2 57	3 51 35					

	Equal altitudes of the Sun.	A. M. h ′ ″	P. M. h ′ ″	Hence the Sun on meridian. h ′ ″	Observed eclipses of Jupiter's satellites. Emersions. h ′ ″	Observations with the meridian telescope.	h ′ ″	Hence the Sun's center on meridian. h ′ ″
May 27						☉'s western limb	11 56 12	
						Eastern ditto	11 58 27	11 57 19½
30						☽'s east limb on meridian.	20 20 31	
31	☉'s sup. limb at 1st hair	7 57 29	3 58 49			☉'s west limb on meridian	11 56 58	
	Ditto at 2d hair	7 58 49	3 57 30	11 58 5½		+Passage of semi-diameter	0 1 8	11 58 6
	☉'s l. limb at 1st hair	8 0 11	3 56 8					
	Ditto at 2d hair	8 1 31	3 54 49					
June 2	Put smaller wires in the telescope. Hence the difference in the intervals	7 57 9	4 0 6					
		7 58 29	3 58 47	11 58 34		☉'s western limb	11 57 26	
		7 59 53	3 57 22			Eastern ditto	11 59 41	11 5 33½
		8 1 13	3 56 3					
3	Transit Day. Equal altitudes were not taken this day, as the instrument was to be otherwise employed in the afternoon.					☉'s western limb	11 57 41	
						Eastern ditto	11 59 57	11 58 49
4			4 1 18					
		7 58 10	3 59 59	11 59 1½		☉'s western limb	11 57 54	
		7 59 34	3 58 35			Eastern ditto	12 0 10	11 59 2
		8 0 54	3 57 15					
5		7 56 43	4 1 50					
		7 58 3	4 0 30	11 59 13½				
		7 59 27	3 59 7					
		8 0 47	3 57 47					
6			2 50 12		1st Satellite.			
			2 48 51	11 59 26	Em. 10 11 2	☉'s western limb	11 58 18	
		9 11 30	2 47 26			Eastern ditto	12 0 33	11 59 25½
7		7 57 52	4 1 25		2d Satellite.	☉'s western limb	11 58 27	
		7 59 16	4 0 1	11 59 36	Em. 8 23 42	Eastern ditto	12 0 44	11 59 35½
		8 0 35				☽'s w. l. on merid.	3 21 53	

June	Observed equal Altitudes of the Sun.	A. M. h ′ ″	P. M. h ′ ″	Hence the Sun on meridian. h ′ ″	Observed eclipses of Jupiter's satellites h ′ ″	Observations with the meridian telescope.	h ′ ″	Hence the Sun's center on meridian. h ′ ″
8	⊙'s up. limb at 1st hair	7 56 27	4 3 12					
	Ditto at 2d hair	7 57 48	4 1 52	11 59 48		⊙'s west limb on merid.	11 58 40	
	⊙'s l. limb at 1st hair	7 59 12	4 0 28			E. limb ditto	12 0 57	11 59 $48\frac{1}{2}$
	Ditto at 2d hair	8 0 32	3 59 7					
10		7 56 22	4 4 1					
		7 57 48	4 2 41	12 0 $9\frac{1}{2}$				
		7 59 12	4 1 17					
		8 0 32	3 59 7					
12						⊙'s west limb	11 59 29	12 0 37
						East ditto	12 1 45	
13		7 59 13	4 2 30	12 0 50	1st satellite.	⊙'s west limb	11 59 42	12 0 $50\frac{1}{2}$
		8 0 33	4 1 11		Em. 12 5 59	East ditto	12 1 59	
14						⊙'s west limb	11 59 57	12 1 5
						East ditto	12 2 13	
16		7 56 52	4 6 16			⊙'s west limb	12 0 26	
		7 58 12	4 4 57	12 1 34		East ditto	12 2 42	12 1 34
		7 59 36	4 3 33			♃'s center on merid.	9 6 4	
		8 0 56	4 2 12					
17						⊙'s west limb	12 0 36	
						+ Passage of semidiameter	1 08,8	12 1 44*
								Thermometer at 77°
						♃'s center on meridian	9 1 50	
19						⊙'s west limb	12 0 56	
						+ Passage of semi-diameter	1 08,8	12 2 4*
								Therm. 77°
						♃'s center on meridian	8 53 24	
21						⊙'s west limb	12 1 17	12 2 $25\frac{1}{2}$
						East ditto	12 3 34	Therm. 83°

Observed

June	Observed equal altitudes of the Sun.	A.M. h ′ ″	P.M. h ′ ″	Hence the Sun on meridian.	Observed eclipses of Jupiter's satellites.	Observations with the meridian telescope.	h ′ ″	Hence the Sun's center on meridian. h ′ ″
22						☉'s west limb East ditto	12 1 28 12 3 45	12 2 36½ Therm. 74½°
23						☉'s west limb East ditto	12 1 39 12 3 55	12 2 47 Therm. 73½°
24						☉'s west limb East ditto	12 1 49 12 4 5	12 2 57 Therm. 84°
25					3d satellite out of the shadow, on applying the eye at 8^h 54′ 39″.	☉'s west limb East ditto	12 1 57 12 4 14	12 3 5½ Therm. 80°
26						☉'s west limb East ditto ☽'s east limb on merid.	12 2 6 12 4 23 18 13 52	12 3 14½ Therm. 85°
27						☉'s west limb East ditto ☽'s east limb on merid. ♃'s center on merid.	12 2 14 12 4 31 19 4 19 8 19 58	12 3 22½ Therm. 88°
28	☉'s up. limb at 1st hair Ditto at 2d hair ☉'s l. limb at 1st hair Ditto at 2d hair	7 59 11 8 0 31 8 1 55 8 3 15	4 7 45 4 6 25 4 5 0			☉'s west limb East ditto	12 2 21 12 4 38	12 3 29½
29		8 0 48 8 2 11 8 3 32	4 7 43 4 6 22 4 4 59 4 3 38		1st satellite appeared from behind a cloud, at 10^h 25′ 1″.	☉'s west limb East ditto	12 2 29 12 4 45	12 3 37 June 30, Therm. 85°

Observed

July	Observed equal altitudes of the Sun.	A. M. h ′ ″	P. M. h ′ ″	Hence the Sun on meridian.	Observed eclipses of Jupiter's satellites	Observations with the meridian telescope.	h ′ ″	Hence the Sun's center on meridian. h ′ ″
					3d satellite. h ′ ″			
2	⊙'s up. limb at 1st hair	8 0 24	4 7 28					
	Ditto at 2d hair	8 1 44	4 6 8		Im. 11 19 36	⊙'s west limb	12 2 52	12 4 0
	⊙'s l. limb at 1st hair	8 3 8	4 4 4[illegible]			East ditto	12 5 8	Therm. 81°½
	Ditto at 2d hair	8 4 29	4 3 23					
3		8 0 46	4 7 21					
		8 2 7	4 6 52			⊙'s west limb	12 2 58	12 4 6½
		8 3 31	4 4 37			East ditto	12 5 15	Therm. 83°
		8 4 51	4 3 16					
4		8 1 9	4 7 13					
		8 2 30	4 5 32			⊙'s west limb	12 3 6	12 4 14½
		8 3 53	4 4 28			East ditto	12 5 23	Therm. 87°
		8 5 14	4 3 8					
5		8 5 36	4 2 57			⊙'s west limb	12 3 11	12 4 19½
						+ Passage of semi-diameter	1 8,5	Therm. at 3 o'clock 94°½
8		8 1 36	4 7 41			⊙'s western limb	12 3 36	12 4 44
		8 2 56	4 6 20			Eastern ditto	12 5 52	Therm. 85°½
		8 4 19	4 4 57					

Table of Eclipſes of Jupiter's Satellites, obſerved at Norriton; compared with the calculated times of the ſame Eclipſes for Greenwich, in order to fix the longitude of the Norriton obſervatory. N. B. If the obſerved times of ſuch of thoſe Eclipſes as have been ſeen at Greenwich ſhould differ from the calculated times, the following difference of longitude, thence deduced, muſt be corrected accordingly.

1ſt ſatellite. Immerſions at Norriton. apparent time.			Calculated apparent time of the ſame for Greenwich.			Long. of Norriton, weſt of Greenwich, thence deduced.
1769.	Day.	h ′ ″		Day.	h ′ ″	h ′ ″
Feb.	16	14 21 10 - - - -	Feb.	16	19 22 29 - - - -	- - - 5 1 19
	23	16 15 1 - - - -		23	21 16 35 - - - -	- - - 5 1 34
April	3	14 49 25 - - - -	April	3	19 51 24 - - - -	- - - 5 1 59
	10	16 46 0 - - - -		10	21 47 14 - - - -	- - - 5 1 14
	12	11 14 39 - - - -		12	16 16 13 - - - -	- - - 5 1 34
May	5	11 29 27 - - - -	May	5	16 31 20 - - - -	- - - 5 1 53
		Emerſions.			Emerſions.	
	21	11 55 13 - - - -		21	16 56 49 - - - -	- - - 5 1 36
June	6	10 11 32 - - - -	June	6	15 12 59 - - - -	- - - 5 1 27
	13	12 5 1 - - - -		13	17 6 31 - - - -	- - - 5 1 30

Difference of longitude from a mean of the above 9 eclipſes - - - - 5 1 34

3d ſatellite, at Norriton; apparent time.				Calculated time for Greenwich.	Hence diff. of long.
	Day.	h ′ ″		h ′ ″	h ′ ″
June	25	8 51 33	Emerſion	13 58 34	5 7 1
July	2	11 15 36	Immerſion	16 12 29	4 56 53
				Mean diff. of longitude	5 1 57

N. B. As the emerſion happened ſo much ſooner, and the immerſion later, than the time given by the Tables, it is concluded, that the ſatellite did not dip ſo deep in the ſhadow as the Tables would have it; but the mean of both gives nearly the ſame difference of longitude as the firſt ſatellite.

" May

" May 20, Mr. Lukens sent up the astronomical
" quadrant, belonging to the East-Jersey proprietors,
" of $2\frac{1}{2}$ feet radius; which I placed in the meridian,
" and observed the following zenith distances of stars,
" to discover the error of the instrument, if any it had.

Observations with the face westwards.		° ′ ″	Ditto with the face eastwards.		° ′ ″
Highest star in the leg of Bootes.	May 31	20 36 6	Same star.	June 6	20 35 55
	June 4	20 36 0		7	20 35 54
	5	20 36 0		8	20 36 0
Arcturus.	May 31	19 46 18	Arcturus.	June 6	19 46 5
	June 1	19 46 14		7	19 46 8
	2	19 46 20		8	19 46 13
	5	19 46 22		10	19 46 11

	Zenith dist. with face of quadrant westwards.	° ′ ″	Zen. dist. face of quad. east.	° ′ ″
The bright star in the Crown.	June 1	12 39 36	June 6	12 39 34
	5	12 39 27	10	12 39 18

" From a mean of the above 18 observations, the
" error of the quadrant is $3''\frac{1}{2}$ to be subtracted from
" the zenith distance, when the face is westwards, and
" added when it is eastward.

Zenith distances of the Sun's limbs observed, and the latitude of the observatory deduced separately from each.

Sun's up. limb;	dist. à zenith. ° ′ ″	Lat. hence deduced. ° ′ ″	Sun's l. limb;	dist. à zenith. ° ′ ″	Lat. hence deduced. ° ′ ″
May 25	18 48 45	40 10 17	June 8	17 29 33	40 9 48
26	18 38 18	40 10 10	9	17 24 35	40 9 47
27	18 28 21	40 10 10	10	17 20 5	40 9 49
June 1	17 43 47	cloudy and doubtful	11	17 15 59	40 9 52
2	17 36 16	40 10 2	14	17 6 9	40 9 58
4	17 21 51	40 9 52			
Doubtful 6	17 8 53	40 9 34			
7	17 3 21	40 9 47			
12	16 41 10	40 10 14			
13	16 37 45	40 10 8			
Mean of the above observations of ☉'s upper limb		40 10 1	Mean of the above 5 observations of ☉'s lower limb		40 9 51
Mean of the 5 obser. of ☉'s l. limb		40 9 51			

The mean of both, viz. 40 9 56 is taken for the latitude of the observatory.

 " The

"The difference of the above obſervations is "greater than might be wiſhed. All that I can of- "fer to excuſe them is the want of better inſtru- "ments; although I think the differences were "much owing to the action of the Sun on the "wooden frame, which ſupported the quadrant. "For I always found, that when the ſhutter in the "roof was opened, the plummet-wire would, in a "minute or two, leave the point, although it had "ſtood quietly over it all the forenoon. Never- "theleſs, a *mean* from ſo many obſervations may be "ſuppoſed very near the truth; ſince, if we leave "out that of June 6, which differs moſt from the "others, the *mean* of them will be but 2″ greater "than the latitude ſet down above."——

So far I have given Mr. Rittenhouſe's account of his obſervations, previous and ſubſequent to the tranſit; for regulating his time-piece, and fixing the latitude and longitude; containing many months work, *viz.* from February 15 to July 8. More obſervations have been taken ſince, but the above are thought ſufficient.

It hath been already mentioned, that it was not till Thurſday afternoon, June 1, that Mr. Lukens and myſelf arrived at Norriton, with a deſign to continue with Mr. Rittenhouſe till the tranſit ſhould be over. When we ſet out, the proſpect before us was very diſcouraging. That day, and ſeveral preceding, had been conſtantly overcaſt with clouds, and frequent heavy rains, a thing not common for ſo long a period at that ſeaſon of the year, in this part of America. But, on Thurſday evening, by one of thoſe ſudden tranſitions which we often experience here, the weather

weather became perfectly clear in lefs than the fpace of one hour, and continued the day following, as well as the day of the tranfit, in fuch a ftate of ferenity, fplendor of funfhine, and purity of atmofphere, that not the leaft appearance of cloud was to be feen in the whole heavens.

June 2, and the forenoon of June 3, were fpent in making the neceffary preparations; fuch as, examining and marking the *foci* of the telefcopes, particularly the reflector with and without the micrometer, and in its different powers. The reflector was alfo placed on a polar axis; and fuch fupports were contrived for refting the ends of the refractors, as might give them a motion as nearly parallel to the plane of the equator as fuch hafty preparations would permit. Several diameters of the Sun were taken during this time, and the micrometer examined by fuch other methods as the time would allow.

The Sun was fo intenfely bright on the day of the tranfit, that it was found beft, early in the forenoon, to lay afide the coloured glaffes, brought with the reflecting telefcope from England; and to put on deeply-fmoaked glaffes, which Mr. Lukens prepared, in their room; and which gave a much more beautiful and well-defined appearance of the Sun.

Mr. Rittenhoufe, on a fuppofition of the Sun's horizontal parallax being but 8″, had, in one of his calculations of the time of the tranfit, made in September, 1768, brought out the firft external contact, at Philadelphia, to be June 3, 2^h 11′, mean time. It happened, that he was not many feconds wrong in this, although moft other calculations made it from 6′ to 8′ later for Philadelphia.

 We

We thought it prudent therefore, at one o'clock, to take off the micrometer from the reflector, which had been uſed with a magnifying power of 95 times; and, after adjuſting the *focus*, continued the ſame power for the reaſons mentioned * below, in order to obſerve the tranſit; and leſt the external contact might happen ſtill ſooner than the earlieſt predicted time, it was reſolved, during the hour from one till two, to keep an alternate watch, through the reflector, on that half of the Sun's limb where the contact muſt happen; while thoſe not thus employed were making all other preparations as follows, *viz.*

1. That each of us might the better exerciſe our own judgment, without being influenced, or thrown into any agitation or ſurprize by the others; it was agreed to tranſact every thing by ſignals, and that one obſerver ſhould not know what the others were doing. The ſituation of the teleſcopes, the reflector being within the obſervatory, and the two refractors, mounted at ſome diſtance from each other without it, favoured this purpoſe. Wherefore,

2. Two perſons, *viz.* Mr. Sellers, one of our committee, for whom no teleſcope could be pro-

* As the refracting teleſcopes gave but a ſmall field, and were very unmanageable, on account of their length, and the Sun's great altitude, it was thought beſt to uſe a ſmaller power and larger field with the reflector, that if the contact ſhould happen at a different part of limb than where it was expected, one of us, at leaſt, might be ſure not to miſs it, but give notice to the others. It was agreed, however, that if the contact happened at or very near the part of the limb where we did expect it, no ſuch notice was to be given. It was, although, thought beſt to have ſome difference in the magnifying powers; and the viſion with the reflector was ſo diſtinct and well defined, that I am well pleaſed I uſed no higher power.

vided,

vided, and Mr. Archibald M'Clean, both men of abilities, and accuftomed to aftronomical obfervations, were placed in one window of the obfervatory, to count the clock, and take the fignals from Mr. Lukens. Two others, who live in Mr. Rittenhoufe's family, and have been trained by him to fervices of this kind, ftood in another window, within the obfervatory, to count the time, and take his fignals. I was within hearing of the beats of the clock, and was to count and fet down my own time.

Thefe preliminaries thus fettled, at two o'clock, each of us applied to our refpective telefcopes; but as there was a great concourfe of many of the principal inhabitants of the county, we were apprehenfive, that our fcheme for filence and order might be interrupted by the impatience and curiofity natural on fuch occafions. We therefore informed the gentlemen, who had honoured us with their company, that the accuracy and fuccefs of our obfervations would depend on our not being difturbed with the leaft noife, till the contacts were over. And to do the company juftice, during the 12 minutes that enfued before the firft contact, there could not have been a more folemn paufe of expectation and filence, if each individual had ftood ready to receive the fentence that was to give him life or death. So regular and quiet was the whole, that, far from hearing a word fpoken, I did not even hear the feet of the four counters, who had paffed behind me from the windows to the clock; and I was furprized, when I rofe up and turned to the clock, to find them all there before me, counting up their feconds to an even number; as I imagined, from

from the deep ſilence, that my aſſociates had yet ſeen nothing of Venus.——

As the contacts are reckoned to be one of the moſt eſſential articles relative to this phænomenon, it is material, before we ſet down their times, to give a particular account of the manner in which each obſerver judged of them, and the other circumſtances attending them.

Mr. RITTENHOUSE's Account of the CONTACTS.

" At 2^h $11'$ $39''$ per clock, the Reverend Mr. " Barton, of Lancaſter, who aſſiſted me at the te- " leſcope, on receiving my ſignal, as had been " agreed, inſtantaneouſly communicated it, by wav- " ing a handkerchief, to the counters at the window, " who, walking ſoftly to the clock, counting as they " went along, noted down their times ſeparately, " agreeing to the ſame ſecond. And three ſeconds " ſooner than this, to the beſt of my judgment, was " the time when the leaſt impreſſion made by Venus " on the Sun's limb could be ſeen through my te- " leſcope.

" When the Planet had advanced about one third " of her diameter on the Sun, as I was ſteadily " viewing its progreſs, my ſight was ſuddenly at- " tracted by a beam of light, which broke through " on that ſide of Venus yet off the Sun. Its figure " was that of a broad-baſed pyramid; ſituated at " about 40 or 45 degrees on the limb of Venus, " from a line paſſing through her center and the

" Sun's

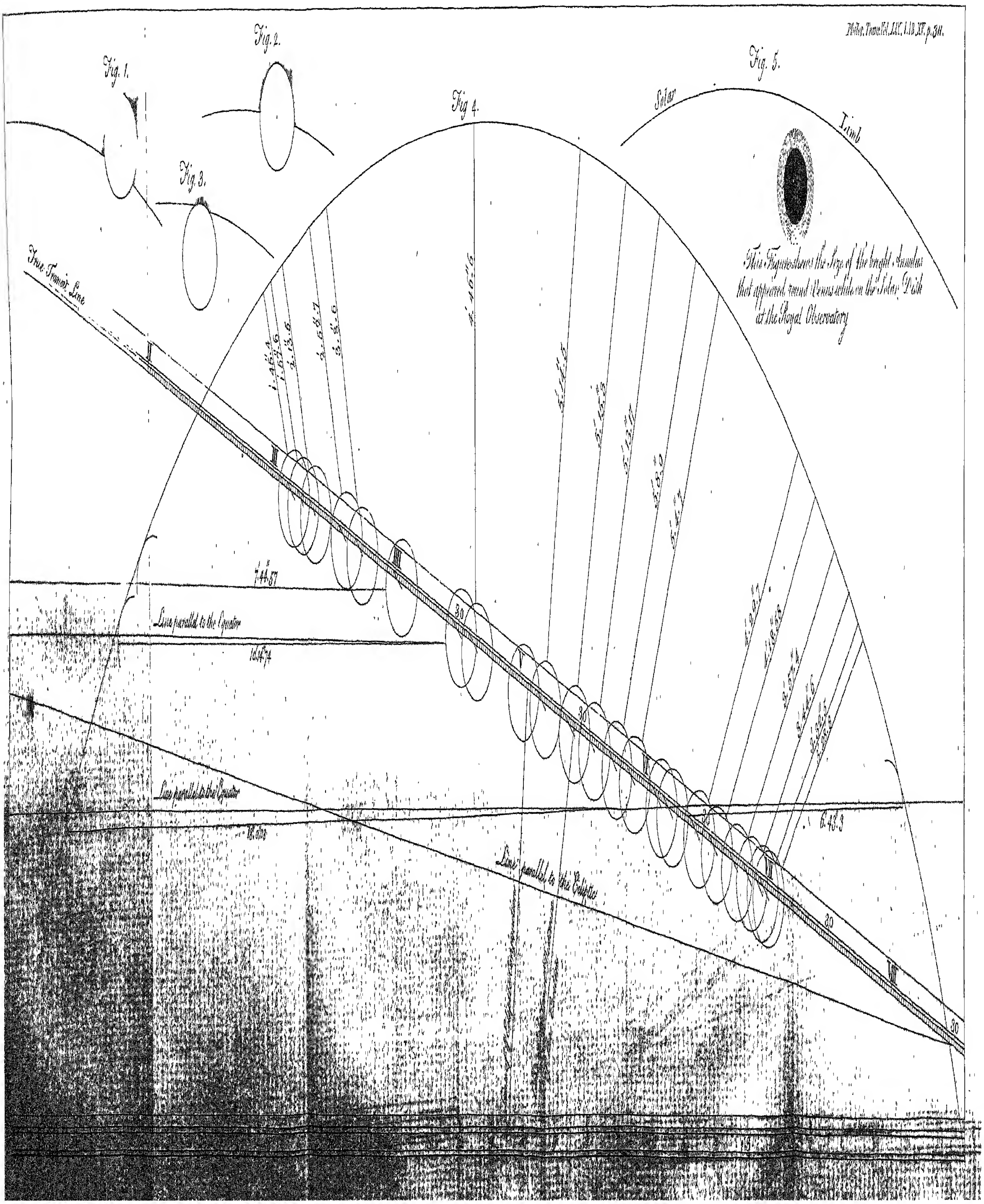

Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Solar
Limb
This Figure shews the Size of the bright Annulus that appeared round Venus while on the Solar Disk at the Royal Observatory
True Transit Line
Line parallel to the Equator
Line parallel to the Equator
Line parallel to the Ecliptic

" Sun's, and to the left hand of that line, as seen
" through my telescope, which inverted. See TAB.
" XV. fig. 1.—About the same time, the Sun's light
" began to spread round Venus on each side, from the
" points where their limbs intersected each other, as is
" likewise represented in fig. 1.

" As Venus advanced, the point of the pyramid
" still grew lower, and its circular base wider, until
" it met the light which crept round from the points
" of intersection of the two limbs: so that when
" half the Planet appeared on the Sun, the other
" half was entirely surrounded by a semicircular
" light, best defined on the side next to the body of
" Venus, which continually grew brighter, till the
" time of the internal contact. See fig. 2.

" Imagination cannot form any thing more beau-
" tifully serene and quiet than was the air during the
" whole time; nor did I ever see the Sun's limb
" more perfectly defined, or more free from any
" tremulous motion; to which his great altitude
" undoubtedly contributed much. When the in-
" ternal contact (as it is called) drew nigh, I fore-
" saw that it would be very difficult to fix the time
" with any certainty, on account of the great breadth
" and brightness of the light which surrounded that
" part of Venus yet off the Sun. After some con-
" sideration, I resolved to judge as well as I could of
" the co-incidence of the limbs; and accordingly
" gave the signal for the internal contact at $2^h\ 28'\ 45''$
" by the clock, and immediately began to count
" seconds, which any one, accustomed to it, may
" do, for a minute or two, very near the truth. In
" this manner, I counted no less than $1'\ 32''$ before

" the

"the effects of the atmosphere of Venus on the "Sun's limb wholly disappeared, leaving that part "of the limb as well defined as the rest. From "this I concluded, that I had given the signal too "soon; and the times given by the other observers "confirm me in this opinion."

Mr. Lukens's Account of the Contacts.

"The telescope I used, being a refracting one of "42 feet, giving but a small field, and something "difficult to manage; I was obliged to move often, "and apprehend that I did not discover the first "external contact exactly. For, after one of those "movements, on bringing the glass to bear again on "that part of the Sun's limb where Venus was ex-"pected, I saw a large tremulous shadow, already "somewhat advanced, and seeming to press still in-"wards on the Sun's limb. Having contemplated "this for a few seconds, and perceiving the appear-"ance grow more dark, and make a better-defined "impression on the limb, I gave the signal to the "persons who counted time for me, which they "noted down separately at $2^h\ 12'\ 3''$ by the clock.

"When Venus was near one half her diameter "advanced on the Sun, I saw a border of light en-"compassing that part of her which was yet off the "Sun. This was so bright, that it rendered that "part of Venus visible, and pretty well defined, al-"though off the Sun. But, towards the internal

" contact, the circular border of light became of a
" more dusky colour, especially at the two points
" where the luminous segments of the Sun's limb
" were ready to close round the Planet. This
" duskiness did not seem to part wholly from the
" Sun's limb, at the time I apprehended the body of
" Venus to be wholly entered on the Sun, and when
" I gave the signal for the internal contact; which
" was noted at $2^h\ 28'\ 58''$ by the clock. And I
" judge from $6''$ to $8''$ more, before I saw the Sun's
" limb clear of this dusky surrounding shadow, and
" as well defined as before the first contact."

Dr. SMITH's Account of the CONTACTS.

" Having, for reasons already assigned, determined
" to continue one of the smaller powers of the Grego-
" rian reflector, for observing the contacts (viz. that
" which we had been using, and were again to use, with
" the micrometer, magnifying 95 times), I had a large
" field, taking in, at least, one half of the Sun's disk; and
" the telescope was so firmly supported (with its axis
" in a polar direction), that it could not be shaken
" by any motion on the earthen floor of the observa-
" tory, and required only a small movement of one
" of the handles of the rack-work to manage it.
" With these advantages, it was easy to keep any
" part of the Sun's limb in the middle of the field,
" without neglecting to cast my eye, every three or
" four seconds, on every other part of the limb

" on both ſides, where there was any poſſibility of
" the contact to happen,

" It being now within about half a minute of the time
" which Mr. Rittenhouſe had calculated for the firſt
" contact, I called to the four counters at the win-
" dows, to be very attentive to thoſe who were to give
" the ſignals from the teleſcopes without doors; and
" turning my eye to that part of the limb where the
" contact was expected, I had been, for ſeveral ſe-
" conds, viewing it ſtedfaſtly, when, all at once, I ſaw
" ſomething ſtrike into it like a watry pointed ſha-
" dow, appearing to give a tremulous motion to that
" part of the Sun's limb, although the teleſcope
" ſtood quite firm, and not the leaſt diſturbance or
" undulation was perceptible in any other part of
" the limb.

" The idea I had formed of the contact was—
" that Venus would inſtantaneouſly make a well-
" defined black and ſmall dent or impreſſion on the
" Sun. But this appearance was ſo different, the
" diſturbance on the Sun's limb ſo undulatory,
" pointed, ill-defined, wateriſh, and occupying a
" larger portion of the limb than I expected, that I
" was held in a ſuſpence of five or ſix ſeconds, to
" examine whether it might not be ſome ſmall ſkirt
" of a watery flying cloud. But perceiving this
" ſhadow, or whatever elſe it was, to preſs ſtill for-
" ward on the limb, with the ſame tremulous,
" pointed appearance (the longeſt points towards its
" middle), I began to count the clock for either
" fifteen or ſixteen ſeconds, when a well defined
" black dent, occupying a leſs ſpace on the Sun's
" limb, became plainly viſible. I then quitted the
" teleſcope,

" telescope, and turned to the clock, to note this
" time for the contact, which was $2^h\ 12'\ 5''$. About
" $22''$ sooner than this (being doubtful to two or
" three seconds at first) was the first impression I
" saw on the limb; which I have marked accord-
" ingly at $2^h\ 11'\ 40''$ to $43''$. If this first impres-
" sion is to be taken for the external contact, I think
" it might be judged of to a single second of time;
" which one could not do by several seconds, either
" with respect to the internal contact, or even with
" respect to the moment of the distinct black dent
" made at the external contact, both which are far
" from being instantaneously perceptible. Whether
" a telescope of larger powers than what I used
" might not have shewn this first impression sooner
" (be it an atmosphere or whatever else), I will not
" determine; though from Mr. Rittenhouse's time
" I think it probably would. But I am sure that I
" saw the first stroke that was perceptible through
" my telescope, and might have noted it to a single
" second, had I expected it in that way.

" As to the internal contact, the thread of light,
" coming round from both sides of the Sun's limb,
" did not close instantaneously, but with an uncer-
" tainty of several seconds, the points of the threads
" darting into each other, and parting again, in a
" quivering manner, several times before they finally
" adhered. I waited for this adherence with all the
" attention in my power, and noted it down for the
" internal contact at $2^h\ 29'\ 5''$, a few seconds later
" than Mr. Lukens, who took the same method of
" judging.

 " Having

" Having quitted my telescope to note down the " time, the gentlemen who counted for us, as well " as several others now come into the observatory, " were anxious to see Venus on the Sun through the " reflector, as it was easily manageable; an indul- " gence not to be denied; and therefore I did not sit " down to it again till about four or five minutes " before the internal contact, and then not with " much attention till the contact was at hand; so " that I saw none of those appearances, on the part " of Venus off the Sun, mentioned by my associates. " But their account may be depended on; for Mr. " Rittenhouse's abilities have been spoken of before; " and few persons have a better judgment, a correcter " eye, or have been more accustomed to view ob- " jects, both celestial and terrestrial, through te- " lescopes, than Mr. Lukens.

" As to the small differences in the times of our " contacts, it is presumed, they may be easily re- " conciled, partly from the different powers of the " telescopes, and partly from the other circumstances " mentioned in the manner of judging of them. " At any rate, we have set them down faithfully.

" As to the first disturbance made on the Sun's " limb, it may he worth considering, whether it was " really from the interposition of the limb of Venus, " or of her atmosphere? The former, one could not " easily imagine it to be, unless her limb and body " were much more uneven than they appeared to " be when seen on the Sun. An atmosphere it " might more probably seem to be, not only from " the faintness of the colour, but the undulatory

" motion,

" motion, which might ariſe from the growing den-
" ſity of the atmoſphere, as it puſhes forward on the
" Sun, varying the refraction of the rays. If ſuch an
" atmoſphere be allowed, then it probably gives the
" ſame tremulous motion, at the internal contact, to
" the thread of light creeping round Venus; and
" prevents its cloſing quietly till the atmoſphere (or
" at leaſt its denſeſt part) be wholly on the Sun; and
" conſequently the true coincidence of the limbs be
" paſt. For though the atmoſphere of Venus can-
" not be ſeen on the Sun, yet that part which is ſur-
" rounding, or juſt entering on the Sun's limb, hav-
" ing, as it were, a darker ground behind it, may be
" viſible. But theſe are only little conjectures ſub-
" mitted to others; though if they have any founda-
" tion, it would make ſome difference in the time
" eſtimated between the contacts."

General Table of the contacts of the limbs of the Sun and Venus, as observed at Norriton, June 3, 1769, reduced to apparent time.

N. B. June 3, by the preceding Tables of the work, the Sun's center was on the meridian, at $11^h\ 58'\ 49''$ by the clock, and June 4, at $11^h\ 59'\ 2''$, and therefore gained $13''$ in 24 hours of apparent time. Wherefore at noon June 3, the clock being $1'\ 11''$ slow of apparent time, it was only $1'\ 10''$ slow at the observation of the contacts. Whence

The apparent time of the different contacts was:

External contact, by Dr. Smith.	h ′ ″ ″	Extern. contact, by Mr. Lukens.	h ′ ″ ″	External contact, by Mr. Rittenhouse.	h ′ ″
1st visible impression on the Sun's limb, in form of a quivering dusky shadow, with many points	2 12 50 to 53 Uncertain to 3″ or 4″.	- - - - - - - - - - - - - - - -		Judged of as described in his account	2 12 49
A well defined black dent in the Sun's limb, at	2 13 15	A small dent in the Sun's limb	2 13 13		
Internal contact.		Internal contact.		Internal contact.	
Judged from a thread or crescent of light, closing round the dark body of Venus, with a tremulous motion, at	2 30 15		2 30 8 to 14	Judged of as described in fig. 3d of his account, Plate XV.	2 29 55

"When Venus was fully entered on the Sun's "limb, and we had satisfied ourselves by comparing "our different notes of the contacts, which were "thrown together on the table of the observatory, we "prepared for the micrometer, and other observa-"tions. The greatest part of the micrometer obser-"vations were taken by me, while Mr. Rittenhouse "undertook to take another set of observations; "namely,

" namely, the appulses of the limbs of the Sun, and
" the center of Venus, to the cross hairs of the equal
" altitude telescope; Mr. Lukens taking and writing
" down the time for him.

The whole observations, reduced to apparent time, are as follows:

N° of observations.	App. time. June 3, 1769. h	′	″	Micrometer measures of the least distance of the nearest limbs of the Sun and Venus. Inches	20th.	500th	Angular value of the micrometer measures. ′	″	Parallax of Venus from the Sun, adjusted to the times of the micrometer observations, for projecting the transit; by Mr. Rittenhouse. In the vertical ″	In the path of Venus. ″	Perpendicular to the path. ″
1	3	7	19	0	4	0,5	1	45,4	14,54	13,67	4,94
R. 2	3	11	39	0	4	12	1	57,6	14,74	13,88	4,96
L. 3	3	17	42	0	5	2	2	13,5	15,09	14,24	5,01
4	3	32	3	0	6	14	2	52,7	15,77	14,92	5,13
R. 5	3	40	4	0	7	4	3	8,6	16,17	15,32	5,23
6	4	35	5	0	10	21,5	4	46,67	18,45	17,45	6,5
7	4	57	9	0	11	19,5	5	11,05	19,02	17,95	6,32
8	5	7	49	0	11	22,75	5	14,5	19,5	18,36	6,63
9	5	21	40	0	11	23,5	5	15,3	19,88	18,64	6,98
10	5	31	46	0	11	21,5	5	13,7	20,12	18,8	7,23
11	5	42	38	0	11	17,5	5	8,93	20,36	18,95	7,48
12	5	51	10	0	11	13,5	5	4,7	20,52	19,06	7,67
13	6	22	4	0	10	5,5	4	29,7	21,0	19,21	8,57
14	6	31	5	0	9	20	4	18,58	21,12	19,22	8,82
15	6	41	24	0	9	0	3	57,38	21,22	19,15	9,14
L. 16	6	48	12	0	8	13	3	44,66	21,26	19,12	9,31
17	6	53	30	0	8	1,5	3	32,47	21,28	19,04	9,49
R. 18	6	56	22	0	7	23	3	28,76	21,29	19,02	9,56

	Distance of the limbs of the Sun and Venus, taken in chords, parallel to the plane of the equator.									Parallax of Venus from the Sun, adjusted as above.		
R. 1	3	58	53	☉'s e. limb	0	17	14,3	7	44,57	17,0	16,1	5,40
2	4	27	18	Ditto	1	3	6	10	14,74	18,16	17,18	5,86
3	6	4	27	Ditto	2	0	20	18	0,08	20,75	19,16	8,06
4	6	9	28	☉'s w. limb	0	15	6,5	6	43,27	20,81	19,2	8,2

N°

Diameters of Venus on the Sun, June 3, 1769.

	No. of observations.	P.M. h	'	Inches	20th	50th
R.	1	3	0	0	2	4,5
	2	3	2	0	2	4,5
L.	3	3	4	0	2	5
	4	4	15	0	2	4,7
	5	5	55	0	2	4,7
R.	6	5	58	0	2	5—

The above times are set down by the clock, according to the vulgar reckoning; as are all the micrometer observations of the Sun's diameters.

Diameters of the Sun, June 2, P. M.

	h	'	''	Inch.	20th	50th	
R.	3	50	40	3	11	15	horizontal
	3	52	0	3	11	14	vertical diameter
	4	37	0	3	11	14,5	
			A. M. June 3.				
R.	8	35	0	3	11	13,5	horizontal diameters.
L.	8	40	0	3	11	16—	
	8	45	0	3	11	13,5	
			P. M.				
	12	35	0	3	11	13	
R.	12	40	0	3	11	12,5	
	4	40	0	3	11	10,5	vertical

From a mean of the above six diameters of Venus on the Sun, allowing for the error of adjustment, as mentioned below:

	'	''
Venus's diameter, for the day of the transit, was	0	57,3
The Sun's semidiameter, from a mean of the five horizontal diameters, taken the same day	15	47,0
Or, from a mean of four, taken that day, leaving out the second, which Mr. Lukens thinks he may have taken too large	15	45,0

All the micrometer observations were separately reduced to their value in minutes and seconds, both by Mr. Rittenhouse and myself. Many more might have been taken; but as so many persons were desirous of looking through the telescope, they could not well be denied; and the number above set down are found fully sufficient for all the purposes of the projection; especially as they have been found to agree so well with each other.

Such of the micrometer measures as were taken by Mr. Rittenhouse or Mr. Lukens, are marked with the initials of their names. All the others I am answerable for.

Our observations being thus finished, Mr. Rittenhouse was pleased to undertake the projection of the

tranſit from them; and his account of the work follows:

Delineation of the tranſit of Venus over the Sun, according to the Norriton obſervations, with the principles of the work. By Mr. Rittenhouſe. See Tab. XV. fig. 4.

" The Sun's horizontal parallax is aſſumed 8″,65 " at his mean diſtance from the earth; from which, " and the obſerved leaſt diſtance of the centers of the " Sun and Venus, the chord for the tranſit line was " laid down. The ſemi-diameters of the Sun and " Venus are taken as by the above obſervations. " One point in the tranſit-line was then fixed by the " firſt micrometer diſtance of the limbs at $3^h\ 7'\ 19''$ " apparent time. This line was carefully divided in- " to hours and minutes, on the ſuppoſition that Ve- " nus moves 240″,36 over the Sun's diſk in an hour, " according to a calculation I had formerly made " from Halley's Tables. The place of Venus's cen- " ter in the tranſit-line was then marked to the times " of each of the micrometer obſervations, and from " thence the *apparent place* of her center found, by " ſetting off the quantity of her parallax from the " Sun in its proper direction. About each of the " centers ſo found, a circle is deſcribed with the rad. " 28″,6, her obſerved ſemi-diameter. Blank lines " were next drawn through the Sun's center, and the " apparent place of the center of Venus. On theſe " the red lines were drawn from the Sun's limb, pre- " ciſely of ſuch length as we found them by the mi- " crometer; ſo that it may be ſeen at once how " far the micrometer meaſures agree with each

 " other,

" other, by obferving how much they exceed or fall " fhort of reaching the limb of Venus. Out of the " 18 that were taken, 14 of them correfpond fo well " that I am convinced they may be depended upon. " The 4th which I have fet down, and only one " other, which will be found omitted, differ fome- " thing from what they ought to be; which might " eafily happen, either from any miftake in noting " the time, or in reading the *vernier* of the micro- " meter, or not fixing it exactly in the direction of " the neareft diftance of the limbs; tho' great care was " taken in this part, by fweeping it conftantly round " to try the truth of the contacts that were formed.

" The meafures intended to be taken in chords " parallel to the equator, are very near the truth, con- " fidering that, in fetting the micrometer to that di- " rection, we had only the truth of the polar axis to " depend on, which was conftructed haftily to an- " fwer the purpofe of the day, and was not exactly " true, as a fmall motion of the rack-work that raifes " or depreffes the telefcope was fometimes neceffary " to keep the Sun in the field. Three of thefe mea- " fures, parallel to the equator, agree with each " other, and with all the reft of the micrometer ob- " fervations, on fuppofing the chord in which they " were taken inclined half a degree to the plane of " the equator. The fourth of thefe meafures is ftill " more nearly parallel to the equator, but diverges a " little the other way. Thefe chords are delineated " in the * projection, and ferve to confirm the other " work.

* It was intended alfo to have confirmed the projection ftill further, by the obfervations made of the appulfes of the limbs of

" All

"All the parallaxes of Venus from the Sun were
"taken from a large projection on a ſcale of half an
"inch to one ſecond, and then reduced to the ſcale
"of this delineation. After calculating ſome of
"thoſe parallaxes, and finding thoſe given by the
"projection conſtantly true to the firſt decimal place,
"any further nicety was thought needleſs.

	°	′	″	
The angle of Venus's viſible way with the ecliptic, I find	8	28	27	
The angle of the ecliptic with a parallel of declination at 3^h P. M.	7	5	13	decreaſing 53″ per hour.
The latitude of the obſervatory, as above laid down	40	9	56	

Hence the parallaxes were fitted to each of the micrometer obſervations, as above.

	h	′	″
If a computation be made with the 1ſt micrometer obſervation of the diſtance of the limbs, we ſhall find the time of the leaſt diſtance of the centers of the Sun and Venus, as ſeen from the earth's center, to have been	5	26	16—

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the Sun and center of Venus to the croſs-hairs of the equal altitude inſtrument; but it was found that ſo many lines would confuſe the figure. And the micrometer obſervations anſwering ſo well, more were thought needleſs. Beſides this, no fractions of ſeconds could be got in the other obſervations; though, nevertheleſs, a good ſeparate projection may be made by them.

 If

		h ′ ″
If a like computation be made from the 16th obſervation, it will be found		5 26 21
By comparing ſome other obſervations with theſe, I conclude, the time of the leaſt diſtance of the centers was		5 26 20

	° ′ ″	
Then as radius to tangent of the angle of the ecliptic, with path of Venus	8 28 27	9,1731571
So is the leaſt diſtance of the centers	610	2,7853298
To the diſtance of Venus's place, when neareſt the Sun's center, from her place at the time of ecliptic conjunction	90,88	1,9584869

	h ′ ″
Which reduced to time (to be ſubtracted) viz.	0 22 41
From time of the leaſt diſtance of the centers	5 26 20
Leaves for the time of ecliptic conjunction	5 3 39

	° ′ ″	
As radius to ſecant of	8 28 27	10,0047676
So is	610	2,7853298
To the geocentric latitude of Venus at the time of ecliptic conjunction	616,73 ″	2,7900974
From the logarithm of which ſubtract the difference of the logarithms of the diſtance of Venus from the Earth and from the Sun		0,4002370
Remains the logarithm of the heliocentric latitude	= 4′ 5″,39	2,3898604

	° ′ ″	
As tangent of inclination of Venus's orbit	3 23 20	8,7724442
To radius; ſo is tangent of Venus's heliocentric latitude	0 4 5,39	7,0754375
To the ſine of her diſtance from node in the ecliptic	1 9 4	8,3029933

The

	S	°	′	″
The Sun's place at the time of the ecliptic conjunction (by Halley's Tab.) was	2	13	26	32
Add the diſtance of the node from the Sun	0	1	9	4
The ſum is the place of the aſcending node of Venus	2	14	35	36
The place of Venus, by Halley's Tables, to the ſame time	8	13	26	22

That is, ten ſeconds too little.

In order to find the error of the micrometer (if any), Jupiter's diameter was meaſured to the right and to the left; and Mr. Rittenhouſe afterwards took the trouble to meaſure the diameter of a white painted circle both ways ten times. This work was performed early in the morning before ſun-riſe, when the air was ſtill, and free from all tremulous motion; the reſult of which, on a mean of thoſe to the right, and a like mean of thoſe to the left, was an error of adjuſtment for the micrometer of 1″,12 to be ſubtracted; which was accordingly allowed for in the reduction of all the micrometer meaſures.

Thus we have given a full and faithful account of our work. We could have wiſhed to have comprized it in leſs room. Had our latitude and longitude been well fixed, as they had been at Philadelphia, by able mathematicians, beforehand, a conſiderable part of our work might have been ſaved. But as it was neceſſary to ſhew, that ſuch pains have been taken in theſe material articles, that they may be depended on; and as we had opportunities

portunities of obſervation, from the goodneſs of the weather, and other circumſtances, which cannot have happened to the generality of obſervers in many parts of the world, we thought we ſhould be the eaſier excuſed by men of ſcience, for the inſertion of twenty ſuperfluous things, than the neglect of any thing material in the account of a phænomenon, which will never be obſerved again by any of the preſent generation of men.

I am,

GENTLEMEN,

with great reſpect,

Your moſt obedient,

humble ſervant,

Philadelphia, July 19, 1769.

William Smith.

N. B. Fig. 5, plate XV, repreſents the appearance of Venus on the Sun to the Reverend Mr. Hitchins, at the Royal Obſervatory. See Philoſ. Tranſ. vol. LVIII. for 1768, p. 363.

XLII. *Obſer-*

XLII. *Observationes Transitus* Veneris *per Discum* Solis, *Die* 3 Junii, 1769, *habitæ in* Suecia, *et* Societati Regiæ Londinensi *communicatæ, a* Petro Wargentin, *ejusdem* Societatis *Sodali, et* Academiæ Regiæ Scient. Stockholmensis *Secretario. Communicated by Mr.* John Ellicott, *F. R. S.*

Read Dec. 16, 1769.

TRANSITUS Veneris per discum Solis, qui contigit die 3 Junii hujus anni, novam et exoptatissimam astronomis præbuit occasionem limatius determinandi veram quantitatem parallaxeos Solis horizontalis, si modo bonæ observationes justis locis habitæ sint. Et cum *Suecia* inter ea terræ loca fuerit, ubi observationes habitæ, collatæ cum aliis in Mari Pacifico, vel *America*, institutis, magni essent ponderis, suo officio deesse neutiquam voluerunt astronomi *Sueci*, quo minus, suo quisque loco, ad rarissimum hoc phænomenon debita cum diligentia attenderet. Sed quoniam et immersio et emersio Planetæ, non nisi in borealibus regni partibus videri potuerit, Academia R. Scientiarum *Stockholmensis* omnino necessarium duxit, mittere duos observatores exercitatos et bonis instrumentis instructos, in *Laponiam*, qui diversis locis transitum hunc opperirentur. Alter, Dominus *Mallet*,

Mallet, Aſtronomiæ Obſervator Regius *Upſaliæ*, villam *Pello* adiit, quæ terminus borealis fuit arcus meridiani terreſtris, ab aſtronomis *Pariſienſibus*, anno 1736, menſurati. Alter, Dominus *Planman*, Philoſophiæ Naturalis Profeſſor in Academia *Aboenſi*, *Cajaneburgum* repetiit, oppidum in confiniis *Finlandiæ* et *Laponiæ* ſitum, ubi priorem Veneris per Solem tranſitum, anno 1761, feliciter notaverat. Iſtas ſtationes Academia *Stockholmenſis* eam præcipue ob cauſam delegerat, quod earum ſitus geographicus jam antea quadantenus determinatus fuerit. Niſi enim longitudo loci certa ſit, obſervatio per ſe optima ſolius immerſionis vel emerſionis Veneris irrita fere eſſet: raræ autem ſunt occaſiones determinandi longitudines locorum, præſertim tempore verno et æſtivo, prope circulum polarem arcticum, ubi eo tempore continua lux Jovem videre et fixarum à Luna diſtantias obſervare vix permittit. Eclipſis quidem Solis, quæ die 4 Junii incidit, videbatur bonam offerre anſam determinandi meridianorum differentias; ſed præterquam quod fieri poſſet, ut qui immerſionem vel emerſionem Veneris feliciter obſervavit, cœlum nubilum haberet ſub eclipſi Solis, notum quoque eſt, obſervationes correſpondentes eclipſium Solis non ſatis certas dare meridianorum differentias. Academia itaque prudentius agere ſibi viſa eſt, eligendo loca ſtationum ante determinata, quamvis Sol, tempore tranſitus, minus ibi eſſet elevatus, quam, pro obtinenda parum majori Solis altitudine, loca magis borealia ſed indeterminata præſcribendo.

Præter dictos duos obſervatores, tertius, Dominus *Hellant*, ipſius urbis *Torneæ* incola, et qui per 30 annos varias obſervationes aſtronomicas ibi dextre

inſtituit,

instituit, ſuam hac quoque occaſione operam pollicitus eſt.

Incidente demum die 3 Junii, D. *Mallet* optimam ſpem habuit bonas nanciſcendi obſervationes, cœlo enim ſereno gaviſus eſt per totam fere diem, immo etiam per noctem ſequentem, ſed nubes circa horizontem borealem omnes ipſius conatus obſervandi momenta immerſionis et emerſionis Veneris, prorſus irritos, proh dolor! reddiderunt. Sub ipſo tranſitu Planetam in Sole aliquoties vidit, ejuſque diametrum ſitumque in diſco, ope micrometri objectivi, menſus eſt; ſed harum obſervationum copiam mihi nondum dedit. Totam quoque Solis eclipſin bene a ſe obſervatam eſſe narrat, ſed hoc debile nimis ſolatium inviſæ Veneris, et vile pretium tanti itineris jure putat.

Hellantius Torneæ, Venerem adhuc minus propitiam expertus eſt. Cœlum enim hiſce diebus fere continuè nubilum habuit, ut ne quidem eclipſin Solis obſervare illi licuerit.

Planmannus autem *Cajaneburgi* felicior fuit. Ibi cœlum tota die præcedente denſis nubibus fuerat obductum, quæ omnem obſervationum ſpem exſtinxerant, ſed inopinato dehiſcebant nubes circa Solem, veſperi, hora 9 8′; tum vero Venus jam cœperat Solem ingredi, et tertia circiter diametri ſui parte intra diſcum Solis erat. Hora 9 20′ 45″$\frac{1}{2}$ tota immergebat, eo enim momento rupta eſt ſubito faſciola, quæ huc uſque Solis et Veneris margines junxerat, et Planetæ corpus lumine Solis undique eſt circumfuſum. Paucis poſt hoc momentum minutis, redibant nubes, quæ noctem largis imbribus et tonitribus fulminibuſque horridam fecerunt, triſtemque aſtronomo noſtro, qui

ſpem ulterioris obſervationis amiſerat. At horâ 3 21′ matutinâ, reſplenduit Sol, nimis licet ſero, Veneris enim pars tertia circiter jamjam emerſerat. Itaque tanto majori cum attentione emerſionem totalem vel contactum limborum exteriorem exſpectabat: contigit ille, horâ 3 32′ 27″, Sole tum ſplendido. Dolendum quidem eſt, cœli faciem non indulſiſſe *Planmanno*, contactum interiorem emerſionis tueri; ſed cum exteriorem bene et certè, tubo 20 pedum optimo notatum afferat, et per experientiam tranſitus anni 1761, compertum ſit, obſervationes contactus exterioris in emerſione fere magis inter ſe convenire et parallaxeos quantitatem non minus certam dare, quam obſervationes correſpondentes contactus interioris, ſperare fas eſt, hanc obſervationem celeb. *Planmanni*, comparatam cum aliis, eximio futuram eſſe uſui, ſi aliis aſtronomis in *Norvegia* et *Ruſſia* non contigit (quod tamen abſit) emerſionem Veneris totam obſervare.

	h	′	″
Die 4 Junii, eclipſeos Solaris initium, cœlo ſereno, notavit D. Planman	9	0	53
Finem	11	0	$0\frac{1}{2}$

Elevatio Poli *Cajaneburgenſis* eſt 64° $13'\frac{1}{2}$. Differentia meridianorum obſervatorii Regii *Grenovicenſis* et *Cajaneburgenſis* eſt 1^h 50′ 47″ quam proxime, quantum ex habitis anno 1761 obſervationibus concludere licet, quam etiam confirmare videntur nuperæ obſervationes immerſionis Veneris, *Cajaneburgi*, *Upſaliæ*, et *Stockholmiæ* factæ, atque inter ſe collatæ.

OBSER-

OBSERVATIONES UPSALIENSES.

Tota die 3 Junii, cœlum *Upſaliæ* fuit ſereniſſimum: nihil itaque aliud obſtitit certitudini obſervationum immerſionis Planetæ, quam undulatio marginis Solaris, quæ, in elevatione duorum vel trium tantum graduum ab horizonte, non potuit non eſſe magna et incommoda.

Upſaliæ quinque obſervatores in immerſionem Veneris intentos habuere oculos animoſque.

	h	′	″
Dominus *Stroemer*, Aſtron. Profeſſor emeritus, uſus eſt teleſcopio reflectente trium pedum.			
Is Solis, Veneriſque margines ſe interius tangentes obſervavit	8	39	58
Sed eos ſeparatos et obſcuram faſciolam eos jungentem ruptam demum vidit	8	40	32
Dominus *Melander*, Aſtron. Profeſſor, tubo 20 pedum, Veneris acceſſum primus notavit	8	22	1
Margines Solis et Veneris ſe intus contingebant	8	39	57
Faſciola margines jungens illi viſa eſt rupta	8	40	12
Dominus *Bergman*, Chemiæ Profeſſor, tubo 21 pedum dictam faſciolam ruptam vidit	8	40	9
Dominus *Proſperin*, Aſtron. Obſervatoris Vicarius, tubo 16 pedum, Venerem exterius tang. vidit	8	22	12
Immerſionem totalem, rupto vinculo obſcuro, obſervavit	8	40	12
Dominus *Salenius*, Philoſophiæ Magiſter, tubo 12 pedum, primum Veneris in margine Solis veſtigium aſpexit	8	22	15
Venerem, undique Solis lumine cinctam, videre ſibi viſus eſt	8	39	46
Sed mox diſparuit illa lux, et Solis margo cum Venere iterum coaleſcebat, vinculo vel faſciola atra cohærentes, quæ demum rupta	8	40	15

Poſtremi hujus phænomeni duplicis ſcilicet immerſionis totalis, explicationem ingenioſiſſimam dedit clariſſ. *Melander*, quam alia occaſione communicabo.

OBSERVATIONES STOCKHOLMENSES.

Heic non tantum undulatio marginum Solis, ſed et nubeculæ prope horizontem, reliquo licet cœlo adhuc nitido, anxios nos reddiderunt de obſervationum ſucceſſu, qui tamen ſpe melior factus eſt, eniteſcente margine Solis ſaltem ſuperiori, iis momentis, quibus maxime opus erat.

	h	′	″
Dominus *Ferner*, Cancellariæ Regiæ jam Conſiliarius, et olim Aſtron. Profeſſor, tubo Dollondiano 10 pedum, diametros ſiderum nonagies augente, primum Veneris in margine Solis veſtigium vidit	8	24	8
Cornua Solis Planetam amplectentia confluebant, ideoque imm. total.	8	41	48
Dominus *Wilcke*, Phyſices Experim. Lector, teleſcopio $1\frac{1}{2}$ pedum egregio, appropinquantem Venerem in ipſo Solis margine conſpexit	8	24	5
Margines Solis et Veneris ſe fere contingere videbantur, quamvis cohærerent	8	41	2
Faſciola margines connectens diluebatur paulatim, conſpicua tamen adhuc erat	8	41	30
Eadem faſciola, tenuior et minus obſcura ſenſim facta, evanuit, et Venerem intra Solem reliquit	8	41	45
Ego, tubo 21 pedum, inter innumeras fluctuantes inæqualitates in margine Solis, conſtantem tandem et atram detexi (quam mox ipſiſſimæ Veneris eſſe notam cognovi)	8	23	51
Margines ſe interius ad ſenſum tangere videbantur	8	41	32
Cohærebant tamen, donec radius Solis fluctuans eos ſeparabat	8	41	47

Ab ortu Solis proximo mane, uſque ad horam $8\frac{1}{4}$ matutinam, cœlum nubibus fuit obductum, et *Stockholmiæ* et *Upſaliæ*.

Stockh. d. 7 Jul.
1769.

XLIII. *Obſer-*

XLIII. *Obſervations of the Tranſit of* Venus *over the* Sun, *contained in a Letter to the Reverend* Nevil Maſkelyne, *Aſtronomer Royal, from Dr.* Alexander Wilſon, *Profeſſor of Aſtronomy in the Univerſity of* Glaſgow.

College, Glaſgow, Sept. 9, 1769.

SIR,

Read Dec. 7, 1769.

I SEND you now the particulars of my obſervation of the tranſit of Venus, together with the obſervations of thoſe who acted in concert with me. I choſe for the place of my obſervations a houſe at ſome diſtance from our obſervatory, but in ſight of it, and more free from the ſmoke of the town; where I had two gentlemen to attend the clock, and mark the times. I carried with me two reflectors of Mr. Short's, which are deſcribed below. Three other inſtruments were made uſe of at the obſervatory; the firſt was an achromatic tube of Dollond's, 29 inch focus, by which an image of the Sun was formed, of about ſix inches diameter, on a board covered with paper. The teleſcope being mounted upon a frame, by which it could be turned about as the Sun moved, and the room properly darkened. This inſtrument was managed by Dr. Williamſon and

and Dr. Reid, at the weſt window of the room of the obſervatory, wherein the aſtronomical clock ſtood; the other two inſtruments were placed without, at the ſouth and north windows of the ſame room, one being a refractor of 13 feet, by which Dr. Irvine obſerved; the other a 12 inch reflector of Short's, by which my ſon obſerved. Theſe two obſervers looked directly at the Sun, having their inſtruments armed with ſmoke-glaſſes; another perſon ſtood at the clock, and counted the ſeconds by coincident beats upon a piece of board, which he held in his hand for that purpoſe, and who named every fifth ſecond, ſo that all the obſervers could hear him diſtinctly. The motion of the clock, made by Shelton, was carefully adjuſted by many tranſits of the Sun and fixed ſtars, over the meridian, both before and after the day of the tranſit; the clock by which my obſervations were made was adjuſted by Shelton's, by means of ſignals made every hour, for ſome hours before and after the tranſit. It was apprehended, that the ſmoke of the town might hurt the obſervations; and, to prevent this as much as poſſible, an advertiſement was put in the news-paper, begging the inhabitants, in caſes where it would not be very inconvenient, to put out their fires from three o'clock that afternoon till ſun-ſetting; the politeneſs of the inhabitants of Glaſgow, in complying with this requeſt, was far greater than could well be expected, inſomuch that there was not a ſpire of ſmoke to be perceived in that quarter from which the obſervations could be incommoded. Having made theſe preparations, we thought we had nothing to fear but the clouds; and indeed the weſtern part of the heavens was covered

with

with thick clouds all the afternoon, till a ſhort time before the external contact; but they drove away towards the north, and left the Sun perfectly bright, excepting that now and then a cloud paſſed over him. But we ſoon found that the conſtitution of the air was otherwiſe unfavourable to our obſervations; the image of the Sun on the white board, made by the achromatic teleſcope, was bright enough; but there was a remarkable undulation in the limb, which could be owing to nothing elſe but the ſtate of the air. This inconvenience was alſo ſenſibly felt both by the other gentlemen and myſelf. Beſides the undulation now mentioned in the limb of the Sun, there was alſo a conſiderable tremor round the planet Venus, when ſhe was ſeen upon the Sun's diſc, and, in conſequence of this, an indiſtinctneſs in her limb, which made it impoſſible to meaſure her diameter by our object glaſs micrometer, or otherwiſe. After the center of Venus had paſſed the Sun's limb, ſhe appeared to us not to be circular, but oblong, the longeſt diameter being that which paſſed through the Sun's center. As the internal contact approached, Venus appeared to us to adhere to the Sun's limb, by a dark protuberance or neck, both the length and breadth of which varied every moment by a conſtant undulation: neither did this neck break off inſtantaneouſly, but changed its colour from black to a duſky brown, till at laſt the interval betwixt Venus and the Sun's limb appeared quite clear. Each of the obſervers wrote down his obſervations on the ſpot. I reduced them, together with my own, to apparent time, from the obſervations I had

had made on the going of the clock, and are as follows:

		h ′ ″
By Doctor Wilson.	External contact	6 54 31,4
	Venus's center judged to be on the limb	7 1 33,4
	Sun's light appeared betwixt Venus and the limb	7 11 56,7
	The beginning of the Solar eclipse next morning, observed by Short's 18 inch reflector	18 30 14,2
	Middle, from a series of observations with the object glass micrometer, fitted to a nine inch reflector of Short's	19 18 47,7
	End not visible	
By Dr. Williamson and Dr. Reid.	External contact	6 54 28
	Internal contact, or when the Sun's light appeared betwixt Venus and the limb	7 12 24
	Venus's center judged to be on the limb, by Dr. Reid	7 1 24

Dr. Reid marked the time when he conceived the internal contact would have happened, if the dark protuberance upon Venus had been taken away, and her disc reduced to a circle, viz. $7^h\ 10'\ 24''$.——He thinks it likewise proper to mention, that, several seconds before the time above set down as the time of the internal contact, he saw a small dint upon the Sun's limb, which he took to be the external contact; that he immediately mentioned this to Dr. Williamson, who happened that instant to have his eyes turned another way; but before he could look at it, it disappeared by the undulation in the Sun's limb. Dr. Reid is the more persuaded that this was not mere imagination, because this dint on the limb of the Sun appeared to him much nearer to the Sun's vertical

vertical diameter than he expected it, but in the very point, however, where it was clearly ſeen immediately after.

		h ′ ″
P. Wilſon.	External contact	6 54 28
	Internal contact	7 12 24

My ſon deſires me to remark, that his firſt obſervation ſhould be conſidered as no other but a poſterior confirmation of Dr. Williamſon's and Dr. Reid's external contact; the fact was, that when theſe gentlemen perceived the firſt contact, their keenneſs made them call out, and it was not till then that he ſaw the phænomenon with perfect certainty. He was conſcious, however, that he fluctuated concerning the reality of the appearance for about twelve ſeconds before that time, during which his determinations were ſuſpended, through an apprehenſion of anticipating the real time, which was heightened by ſo cloſe a neighbourhood with the other obſervers, all of whom he could not help being ſenſible were ſtill expecting the phænomenon. Upon the whole, he is rather of opinion that he would have put down the external contact at leaſt eight ſeconds ſooner had he been obſerving apart. His ſecond obſervation, by which he means the inſtant when the interval between Venus and the Sun's limb firſt appeared obvious, was taken down without the leaſt knowledge of what was paſſing among the other gentlemen who obſerved. Dr. Irvine has been out of town for ſome conſiderable time paſt, and forgot to lodge his obſervations with me, but I remember certainly that he made the external contact three ſeconds ſooner than the reſt; but his internal contact was ſome ſeconds

 later,

later, but how many I do not now remember. Mr. Anderſon, F.R.S. fitted up a clock and apparatus in the college ſteeple; his clock was regulated as above, by ſignals from the obſervatory; he obſerved the tranſit with a large reflector, and his aſſiſtants obſerved with refractors: they were all of them uncertain about the external contact, owing to the ſtate of the atmoſphere, and a tremor given to the ſteeple by the wind; but none of their other obſervations varied, above three ſeconds, from my own, as related above.

	°	′	″	
Latitude of the obſervatory	55	51	32	
Longitude by correſponding obſervations	0	17	11	of time from Greenwich W.

I am, Sir,

with great eſteem,

Your moſt obedient ſervant,

Alexander Wilſon.

I have obſerved, during the courſe of this year, ſeveral times, the Aurora Borealis form itſelf into an arch, 20 or 30 degrees above the horizon, which continued permanent for ſome time; and in this caſe the vertex of the arch appeared always to be weſt from north, by about the variation of the needle in 19 or 20 degrees. Whether or not it is always ſo, I cannot yet ſay.

XLIV. *An*

XLIV. *An Account of the late Transit of* Venus, *observed at* Hawkhill, *near* Edinburgh. *In a Letter to the Astronomer Royal, from* James Lind, *M. D. at* Edinburgh. *To which are added some Remarks by the Astronomer Royal; and further Particulars relative to the Observations communicated in other Letters.*

Hawkhill, 5 June 1769.

SIR,

Read Dec. 7, 1769.

I HAVE the pleasure to transmit to you the account of our observations at this place. James Hoy, our young observer, observed in the house, on the ground-floor, in the room with the house-clock, with the $3\frac{1}{2}$ feet achromatic telescope with triple object glass; Lord Alemoor observed on the floor above, with the 18 inch reflector, and a watch that shewed seconds, set a few minutes before the transit began, and compared after each contact; I was in the observatory, where I used my own 2 feet achromatic telescope, a mathematical instrument maker counting seconds from the clock. The following is the account of all our observations:

	Ext. cont.			Int. cont.			
	Mean time.						
	h	′	″	h	′	″	
Lord Alemoor	6	57	33	7	14	32	18 inch reflector
James Hoy	6	57	30	7	14	35	$3\frac{1}{2}$ f. achromatic, mag. 150
Dr. Lind	6	57	41	7	14	37	2 f. achromatic, magn. 100

In the internal contact, James Hoy differed from the other gentleman and me two minutes, he calling it 12 minutes, and we 14 minutes; which of us is wrong, will be no difficult matter to determine. In the internal contact we all obſerved the black ligament or protuberance, which was not broke for ſome ſeconds after the regular circumference of Venus ſeemed to be within the Sun; and the obſervation we ſend you was, as near as we could judge, about the time this protuberance was going to break. Lord Alemoor alſo, and he only, obſerved regular circumferences of the Sun and Venus in contact, at $7^h\ 14'\ 10''$, mean time.

The morning promiſed ill, yet we got 9 very good altitudes of the Sun near the prime vertical. About noon the day was terrible, with thick clouds, and like ſettled rain. You may imagine how we felt. About two o'clock the wind began to change from the ſouth to the weſtward; about three o'clock it was weſt, and the clouds breaking; ſo that we got 5 very good correſponding altitudes. There was, about 4 o'clock, a very hard thunder ſhower, and calm, after which the wind began to blow briſkly from the north-weſt; the clouds blown away, and thoſe near the horizon depreſſed and held down, the Sun ſhone clearer than I ever ſaw it, and not a cloud was to be ſeen in that quarter. It remained ſo till after both contacts; when, not half a minute after, ſmall flying clouds paſſed over the Sun, and ſhewed us how much we were obliged to kind heaven for the very favourable opportunity we had of making our obſervations. It appeared, I aſſure you, as if Providence had withdrawn the clouds over head, and held down thoſe near the

the horizon, for that very purpoſe. The night continued equally clear and ſerene, as did the morning, till after the eclipſe; half an hour after which it began to overcaſt, and put on the ſame cloudy appearance it has wore for ſome months paſt. Although the morning was ſo favourable, yet we loſt the beginning of the eclipſe, from being too long in getting to our poſts; however, I here ſend you the contacts, with the different ſpots of the Sun, and its end moſt exactly. If you obſerved the ſpots, it will, I imagine, be as exact as if the beginning and end only had been obſerved.

I am,

SIR,

Your moſt obedient,

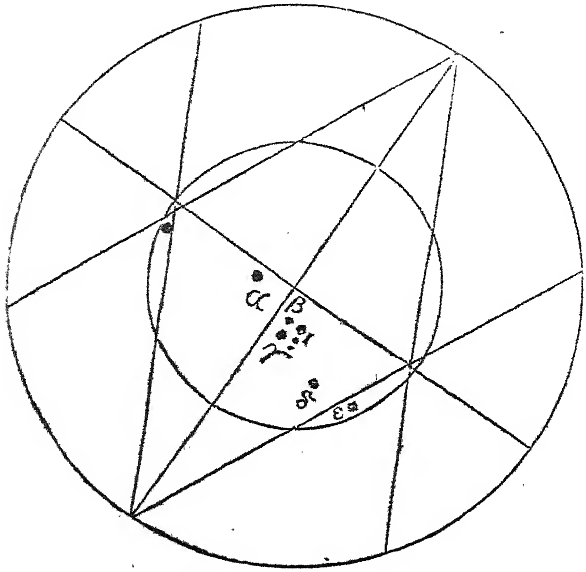
humble ſervant,

James Lind.

P. S. I hear the day proved alſo very favourable at Glaſgow.

Mean

Mean times of the contacts of the Moon's limbs, with the spots of the Sun, June 3, 1769.



			Mean time. h	′	″
☾'s S. E. limb with	α	Ext. contact	18	47	33
	α	Center	18	48	1
	α	Int. contact	18	48	18
	β	Center	18	59	20
	γ	Ditto	19	1	18
	δ	Ditto	19	9	9
		Ditto	19	18	23
☾'s W. limb with	α	Int. contact	19	37	18
	α	Center	19	37	47
	α	Ext. contact	19	38	15
		Center	19	51	16
	γ	Ditto	19	52	47
	δ	Ditto	20	3	46
	ε	Ditto	20	15	9
End of the eclipse			20	17	30

Remarks

Remarks by the ASTRONOMER ROYAL.

Hawkhill is ſaid by Dr. Lind to be about 1½ miles N.E. of Edinburgh. It is the ſeat of Lord Alemoor, one of their judges, who is fond of aſtronomy, and has built a ſmall obſervatory there with a moveable roof, upon Mr. Smeaton's plan, which I ſent to Dr. Lind. The correſponding altitudes, for determining the time of the obſervations of the tranſit of Venus, were taken, by reflection, from a baſon of quickſilver or treacle, with a braſs Hadley's ſextant, made by Mr. Ramſden; the ſurface of the fluid being defended from the wind by a glaſs ground truly plane. They find that the equal altitudes ſeldom differ above two or three ſeconds in determining the time of noon; ſo that, by taking a great many at once, and taking the mean, they think they cannot fail of coming very near the truth. I have examined the equal altitudes made about the time of the tranſit, and the times of the contact are given corrected in the foregoing account. The clock in the obſervatory ſeems to go pretty well, though it only beats dead quarter ſeconds; it has a mahogany pendulum, and was made by Mr. Cummins. In the houſe was a clock beating ſeconds, and ſet, by means of the other, in the afternoon, before the beginning of the tranſit. The latitude of the place was alſo determined by meridian altitudes, taken by reflection with the ſextant, and, by the mean of 10 obſervations, which all agree within the compaſs of ,2 minutes, is 55° 57′ 37″ N. The end of the Solar eclipſe was obſerved

obſerved by two perſons with the two achromatic teleſcopes, with treble object glaſſes, and they agreed to a ſecond.

Dr. Lind writes, another time, that, being from home, at Lees, near Coldſtream, 7 miles weſt of Berwick, he obſerved the latitude of the place about 55° 37′.

The foregoing particulars are extracted from letters received from Dr. Lind. He has alſo communicated to me the following obſervations of the tranſit of Venus and Solar eclipſe, made by the Reverend Mr. Brice, at Kirknewton, as follows:

I here likewiſe ſend ſome obſervations, made in our neighbourhood, by the Reverend Mr. Bryce, Miniſter at Kirknewton. He is a very good aſtronomer, and is a writer in the Philoſophical Tranſactions. Kirknewton is in lat. 55° 54′ 30″ N. and about 17 miles W. of Hawkhill, from meaſuring it on Lawrie's map of the environs of Edinburgh.

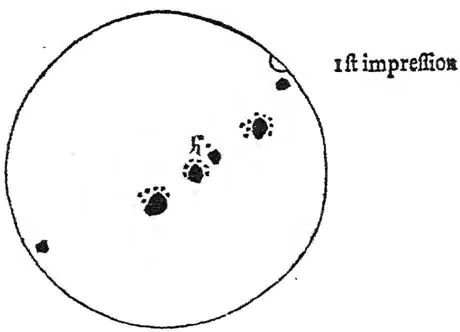
The clock had been tried by ſeveral tranſits of a fixed ſtar, and always found to meaſure time ſo exactly, that in the ſpace of five days it did not differ one ſecond from the truth; it was alſo examined by taking equal altitudes of the Sun, and found to be 18″ ſlow. The day was cloudy, with flying ſhowers, till about two o'clock in the afternoon; then it grew ſomewhat clear, and about four the Sun ſhone out exceeding bright, when I obſerved carefully the ſpots upon the Sun; the brightneſs continued till about 15′ before 7^h, when a cloud came over the Sun, which was not ſeen till 6^h 55′ 40″ mean time, as ſhewn by the clock, and then Venus had made a ſenſible

ſenſible impreſſion upon the upper limb of the Sun's zenith, and $\frac{1}{10}$th, as I judged, upon the Sun.

	h	'	''
Half on the Sun, as we thought,	7	3	55
Internal contact clearly ſeen	7	11	55
18'' added for the clock too ſlow		+	18
	7	12	13
And if Venus takes 19' from the firſt impreſſion to the internal contact, the tranſit began at	6	53	13
Seen going till the Sun ſet in a cloud near the horizon	8	24	39

When near the horizon, Venus's edge was full of notches and protuberances, and ſhe appeared as if moving round like a wheel.

Eclipſe of the Sun, 4 June, common reckoning.



	h	'	''
(1) Beginning of the eclipſe	6	27	50
(2) The 1ſt impreſſion made a little above the line of the ſpots upon the Sun's diſk			
(3) The Moon's edge touches the great ſpot	6	56	5
(4) 1ſt cluſter of ſpots touched	6	57	0
(5) Covered at	7	0	0
(6) Cuſps of the Sun upon an horizontal line	7	6	45
(7) Another ſpot touched	7	7	25
(8) Spot on the Sun's north limb touched	7	16	44
(9) 1ſt ſpot of the cluſter emerged	7	50	20

(10)	2d ſpot of the cluſter emerged	7	50	40
(11)	3d ſpot of ditto emerged	7	51	32
(12)	Spot on the north limb emerged	8	13	40
(13)	Eclipſe ended	8	15	50

(14) Laſt impreſſion made at the ſpot upon the north limb of the Sun, and the whole eclipſe ſeen very diſtinctly, from the beginning to the end.

(15) An evident irregularity in the under edge of the Moon, which entered upon the ſouth ſide of the Sun, and traverſed it from ſouth to north.

(16) Fahrenheit's thermometer ſtood all the while, in the ſhade, at $53°\frac{1}{4}$, and continued ſo for ſeveral hours after.

N. B. The clock 18″ ſlow to be added to the ſeveral obſervations.

N. B. The internal contact is, when the thread of light was compleated.

From the above obſervation, and from every one of any credit, we Hawkhill gentlemen are late in the external contact.

XLV. *Obſer-*

XLV. *Obſervation of the Tranſit of* Venus, *and other Aſtronomical Obſervations, made at* Gibraltar; *contained in a Letter to the Aſtronomer Royal from Lieutenant* Jardine.

To regulate the clock.

Read Dec. 7, 1769.

AN equal altitude inſtrument was fixed (nearly ſuch as is deſcribed in Smith's Optics, vol. II. p. 328), on which was mounted a ſmall teleſcope with croſs hairs.

		Sun's upper limb touched the horizontal hair at			Sun's lower limb touched the horizontal hair at		
		h	′	″	h	′	″
June 1	Morning	8	32	43	8	35	8
	Afternoon	3	24	33	3	22	0
2	Morning	8	32	20	8	34	46
	Afternoon	3	25	4	3	22	36
4	Morning	8	32	27	8	35	13
	Afternoon	3	25	27	3	22	47
5	Morning	8	32	35	8	35	10
	Afternoon	3	25	53	neglected		

TRANSIT OF VENUS.

By three obſervers, with two $7\frac{1}{2}$ feet refractors, and one 2 feet reflecting teleſcope.

	h	′	″
June 3, Venus's 1ſt external contact with the Sun, at	6	49	58
1ſt internal contact with the Sun, at	7	7	11
Sun ſet behind a hill	7	8	3
Clock before mean time	0	1	8,8

For the LATITUDE of the place.

Obſerved by a Hadley's quadrant, divided into minutes, the double meridian altitudes of Jupiter and Cor Scorpii, reflected from water.

Double meridian altitude of Jupiter.

		°	'	"
May	30 =	77	6	
June	14 =	77	50	
	17 =	77	55	30
	21 =	78	3	30
By another obſerver,	=	78	3	0
June	22 =	78	4	0
	29 =	78	10	45
July	4 =	78	14	0

Double meridian altitude of Cor Scorpii.

		°	'	"
June	28 =	56	7	
	29 =	56	5	
July	7 =	56	6	30
	8 =	56	5	0

Clear weather, in general, during theſe obſervations; and Fahrenheit's thermometer, in the middle of the day, between 75° and 80°
in the night, between 68 and 71

For the LONGITUDE of the place.

			h	'	"	
May	30	Emerſion of Jupiter's firſt ſatellite	12	59	56	
		Clock before mean time	0	1	2	
May	31	Emerſion of Jupiter's 2d ſatellite	10	51	51	
		Clock before mean time	0	1	$3\frac{1}{2}$	
June	8	Emerſion of Jupiter's firſt ſatellite	9	22	34	These two are moſt to be depended upon.
		Clock before mean time	0	1	$16\frac{1}{2}$	
June	15	Emerſion of Jupiter's firſt ſatellite	11	15	$54\frac{1}{2}$	
		Clock before mean time	0	1	28	
June	25	Immerſion of Jupiter's 3d ſatellite	11	59	56	uncertain to perhaps 5 or 6"
		Clock before mean time	0	1	44	
July	8	Emerſion of Jupiter's firſt ſatellite	11	30	57	
		Clock before mean time	0	2	4	

ECLIPSE OF THE SUN.

		h	′	″	
June 4	First contact at	6	6	54	seen perhaps a little too late.
	Last contact at	7	19	28	exact.
	Clock before mean time	0	1	9	

Elev. of ☉'s l. limb by Hadley's quad. at { 1st contact 14° 41′ / 2d contact 28 55 } both exact.

Dip of the horizon, for 160 feet above the level of the sea, is to be allowed.

SIR,

I Have been disappointed in the pleasure I promised myself, when I saw you, of observing some occultations of fixt stars, by the Moon, &c. We shall be glad, if these observations can be of any service. We have endeavoured to discover to you the degree of dependance to be placed thereon.

With regard to the clock, we conclude from these equal altitudes (correcting for difference of declination, &c.), that, on the 3d of June, it was before mean time 1′ 8″,8. You will easily discover if there is any error. We afterwards regulated by frequent equal altitudes, by a meridian line on the bottom of a window, and by the setting of stars behind some solid buildings.

The latitude appears, from these double altitudes, to be somewhere between 36° 3′, and 36° 4′; but from more correct declinations, &c. you will be able to determine it more precisely.

For the eclipse, we had no micrometer, nor any other method of determining the quantity of it.

 To

To the eye, between $\frac{1}{4}$th or $\frac{1}{5}$th of the Sun's diameter ſeemed to be eclipſed. Though the beginning be rather incorrect, the end may be depended on.

We ſhall be glad if you can determine, from hence, the latitude and longitude of the place, and to know the reſult of the diſcoveries made in our ſyſtem, by the obſervations of the late tranſit, if you will favour us.

I am,

SIR,

Your moſt obedient,

humble ſervant,

Gibraltar, July 14, 1769.

Alexander Jardine.

By re-computing theſe obſervations, I find, that the external contact of Venus happened at $6^h\ 51'\ 8''$, the internal contact at $7^h\ 8'\ 21''$, the beginning of the eclipſe of the Sun at $18^h\ 8'\ 0''$, and the end at $19^h\ 20'\ 33''$, all apparent time; and that the latitude of the place, by the mean of the 4 altitudes of Cor Scorpii, is $36°\ 4'\ 44''$, N. The dip of the horizon of the ſea, for an elevation of 160 feet, may be reckoned $12'\ 5''$.

Nevil Maſkelyne.

XLVI. *Obſer-*

XLVI. *Obſervations of the Tranſit of* Venus *over the* Sun, June 3, 1769. *In a Letter to the Reverend* Nevil Maſkelyne, *F. R. S. Aſtronomer Royal, from* John Winthrop, *Eſquire, F. R. S.* Holliſian *Profeſſor of Mathematics at* Cambridge, *in* New England.

Cambridge, New-England, Sept. 5, 1769.

REVEREND SIR,

Read Dec. 7, 1769.

I BEG leave to lay before you my obſervation of the late tranſit of Venus, the beginning of which I obſerved, in this place, with all the care I was capable of. Our ſituation on this North American coaſt, I am ſenſible, was not the moſt favourable, as we could ſee only the firſt part of the tranſit; yet, I preſume, careful obſervations, wherever made, will have their uſe in determining the grand problem of the Sun's parallax; at leaſt, may ſerve as collateral evidences to the capital obſervations.

Our apparatus having been wholly deſtroyed by fire, about five years ago, we have ſince procured a new ſet of aſtronomical inſtruments, made by ſome of the moſt eminent hands in London. A clock, by Mr.

Mr. Ellicott, with the pendulum of his invention, having the bob ſupported by levers. An aſtronomical quadrant, of 2 feet radius, made by Mr. Siſſon; and an equal altitude inſtrument, by Mr. Bird; each having three horizontal wires in the teleſcope. A reflecting teleſcope, of four feet focus; another of two; and another of 1 foot; fitted with an object glaſs micrometer, of $21\frac{1}{2}$ feet focus; all three made by the late Mr. James Short.

I was obliged to remove the clock to another apartment, for the ſake of the tranſit, which I did on the 23d of May, when I took ſome equal altitudes. By reaſon of an almoſt continual ſucceſſion of cloudy weather till the end of that month, I could make but few material obſervations afterwards for regulating the clock. But, happily, the weather cleared up on the 1ſt of June, about noon, and continued fine for ſeveral days. As the preciſe knowlege of the true time is of the utmoſt conſequence in the preſent caſe, I ſuppoſe a particular account of the obſervations made for this purpoſe, within a few days of the tranſit, will be moſt ſatisfactory to you; and this muſt be my apology for the prolixity of the detail.

Obſervations for regulating the clock.

1769 June

♃ 1 Very cloudy till noon; the Sun hardly viſible for a minute at a time; ſo that I could make only the following ſparſe obſervations.

Equal altitudes of the Sun's limbs.

	Morning. h ′ ″	Afternoon. h ′ ″	Middle Times h ′ ″
I.	7 55 13	4 0 6	11 57 39,5
			equation — 4,2
			11 57 35,3 ☉ on the meridian
II.	8 13 36	41 43	—11 57 39,5
	14 6	3 41 13	equation — 3,9
			11 57 35,6 ☉ on the meridian
III.	8 21 21	3 33 57	11 57 39
			equation — 3,8
			11 57 35,2 ☉ on the meridian

☉ on the meridian.	h ′ ″
I.	11 57 35,3
II.	35,6
III.	35,2
Mean	11 57 35,4—

or, true time of noon by the clock.

♀ 2

I.

		Morning	Afternoon	Middle Times
☉'s sup. limb	1	8 3 39	51 49	44
	2	4 49	50 38	43,5
	3	6 0	49 28	44
low. limb	1	6 30	48 58	11 57 44
	2	7 41	47 46	43,5
	3	8 52	3 46 35	43,5
			Mean	11 57 43,75
			Equation	−3,77
			☉ on meridian	11 57 40

II.

		Morning	Afternoon	Middle Times
☉'s sup. limb	1	8 11 47	43 41	44
	2	12 58	42 31	44,5
	3	14 9	41 19	11 57 44
low. limb	1	14 39	40 49	44
	2	15 50	39 38	44
	3	17 1	3 38 27	44
			Mean	11 57 44,1
			Equation	−3,7
			☉ on meridian	11 57 40,4

1769 June			Morning.	Afternoon.	Middle times.
			h ′ ″	h ′ ″	h ′ ″
			III.		
☿ 2	☉'s up. limb	1			
		2	8 35 42	19 45	43,5
		3	36 54	18 33	43,5
	low. limb	1	37 25	18 2	11 57 43,5
		2	38 37	16 51	44
		3	39 48	3 15 40	44
				Mean	11 57 43,7
				Equation	−3,3
				☉ on meridian	11 57 40,4
			IV.		
	☉'s up. limb	1	8 41 49	13 37	43
		2	43 1	12 25	43
		3	44 13	11 14	11 57 43,5
	low. limb	1	44 45	10 43	44
		2	45 57	9 30	43,5
		3	47 10	3 8 17	43,5
				Mean	11 57 43,4
				Equation	−3,2
				☉ on meridian	11 57 40,2

	☉ on the meridian. h ′ ″
I.	11 57 40
II.	40,4
III.	40,4
IV.	40,2
Mean of all	11 57 40,3

or, true time of noon by the clock.

			Morning.	Afternoon.	Middle times.
			I.		
♄ 3	☉'s up. limb	1			
		2	7 46 28	9 12	
		3	47 39	8 1	11 57 50
	low. limb	1	48 9	7 31	equat. −3,8
		2	49 20	4 6 20	
		3			
				☉ on meridian	11 57 46,2
			II.		
	☉'s up. limb	1	7 51 38	4 2	50
		2	52 48	2 51	49,5
		3	53 59	1 40	11 57 49,5
	low. limb	1	54 29		
		2	55 40	4 0 0	50
		3	56 50		
				Mean	11 57 49,75
				Equation	−3,74
				☉ on meridian	11 57 46

1769. June ♄			Morning. h ′ ″	Afternoon. h ′ ″	Middle times. h ′ ″
			III.		
	☉'s up. limb	1	7 58 51	56 46	48,5
		2	8 0 2	55 35	48,5
		3	1 13	54 34	11 57 48,5
	low. limb	1		53 55	
		2	2 54	52 46	50
		3	4 5	3 51 35	50
				Mean	11 57 49,1
				Equation	−3,6
				☉ on meridian	11 57 45,5

☉ on the meridian.

I.	11 57 46,2
II.	46
III.	45,5
Mean of all	11 57 45,9

or, true time of noon by the clock.

At 4^h 15′ thermometer 85¾.

			I.		
☉ 4	☉'s up. limb	1	7 45 1	10 51	56
		2	46 12	9 40	56
		3	47 23	8 29	11 57 56
	low. limb	1	47 53	7 59	56
		2	49 4	6 48	56
		3	50 14	4 5 37	55,5
				Mean	11 57 56−
				Equation	−3,6
				☉ on meridian	11 57 52,4
			II.		
	☉'s up. limb	1			
		2	7 52 32	3 20	56
		3	53 42	2 9	55,5
	low. limb	1	54 12	1 40	11 57 56
		2	55 23	4 0 29	56
		3	56 34	3 59 18	56
				Mean	11 57 56−
				Equation	−3,5
				☉ on meridian	11 57 52,5
			III.		
	☉'s up. limb	1	7 57 38	58 11	54,5
		2	58 50	57 0	55
		3	8 0 2	55 49	11 57 55,5
	low. limb	1	0 52	55 19	55,5
		2	1 42	54 8	55
		3	2 43	3 52 57	55
				Mean	11 57 55,1
				Equation	−3,4
				☉ on meridian	11 57 51,7

☉ on the meridian.

I.	11 57 52,4
II.	52,5
III.	51,7
Mean of all	11 57 52,2

or, true time of noon by the clock.

Observation

Obſervation of the TRANSIT of VENUS.

I choſe to obſerve the tranſit with the 2 feet teleſcope, as I ſuppoſed moſt of the obſervations in other parts would be made with teleſcopes of that ſize; and I uſed a power that magnified 90 times, which gave a very diſtinct view of the ſpots then on the Sun, Soon after two o'clock, I began to look on the Sun's upper limb, where the Planet was to enter. The firſt impreſſion I perceived was at $2^h\ 27'\ 51''$, by the clock, the Sun being then perfectly clear. I then reſted my eye, which was pretty much fatigued, to prepare it for the total ingreſs or interior contact. At $2^h\ 45'\ 15''$, I began to be doubtful whether the internal contact was not formed; but at 20'' was ſatisfied that it was paſt, the Sun's limb being reſtored to its integrity, in the place where it had been interrupted by the Planet. During this interval of near 5'', there ſeemed to be a duſkiſhneſs in the place of contact; my idea of which is well repreſented by Mr. Dunn's figure of what he calls the grey contact, in Phil. Tranſ. Vol. LII. Tab. VII. p. 190.

By the foregoing equal altitudes it appears, that the clock was now $2'\ 13''+$ too ſlow. I therefore ſtate the obſervation as follows:

	Apparent time.
	h ′ ″
Firſt viſible impreſſion of Venus upon the Sun	2 30 4
Internal contact	47 30

This time of internal contact, I think, cannot differ above 2'' from the truth, and perhaps may not differ

differ 1″. But about 4″ of Venus's diameter muſt have entered upon the Sun before I perceived the impreſſion. At nine in the morning, I obſerved the Sun's diameter, in the horizontal direction, to be 1 21 1 parts of the micrometer, = 31′ 33″,2. At 5^h 34′ 23″, the Sun's north limb was diſtant from Venus's ſouth limb 9 3 of the micrometer, = 6′ 16″,2. At 5^h 37′ 23″, I found no ſenſible difference; and the Sun's north limb was then diſtant from Venus's north limb 7 14½ of the micrometer, = 5′ 17″,6. This gives Venus's diameter 58″,6; and the leaſt diſtance of centers 9′ 59″,7. Hence, the true duration of the ingreſs ſhould be 18′ 56″; but this being here contracted 15″, by parallax, is reduced to 18′ 41″. So that the firſt contact, ſtrictly ſo called, happened 1′¼ before the impreſſion was diſcovered; and the central ingreſs was at 2^h 38′ 5″. The neareſt approach was nearly, I ſuppoſe, at 5^h 37′.

After Venus was entered upon the Sun, I viewed her attentively ſeveral times, with a power of the great teleſcope which magnified 260 times, but could perceive no ſuch duſkiſhneſs round her as I ſaw at the internal contact, nor that imperfect light upon her diſk, eſpecially near the centre, which Mr. Dunn ſpeaks of; neither could I diſcover any ſatellite. Soon after ſix, the weſtern ſky began to be over-caſt, ſo that for a conſiderable time before ſun-ſet the Sun was hid.

I made obſervations for determining ſome other poſitions of Venus upon the Sun; but as they can be of no ſervice in the grand problem of the parallax, I think it needleſs to ſwell this letter, very long already, with them. I therefore only add, that the latitude of this

this place is nearly 42° 25′ N. and the difference of meridians west from London about $4^h\ 44'$. But this may be farther ascertained by the following emersions of Jupiter's satellites, which I observed with the 2 feet reflector.

Emersions of Jupiter's first satellite.

		Apparent time. h	′	″
1768	April 25	9	13	52
	May 18	9	27	27
	June 10	9	37	25
	July 3	9	45	54
1769	May 14	10	19	7
	Aug. 23	7	31	50

Emersion of ♃'s second satellite.

		h	′	″
1769	June 7	9	1	15

I submit the foregoing observations to your candid acceptance; and am, with great respect,

REVEREND SIR,

Your most obedient humble servant,

John Winthrop.

XLVII. Of

XLVII. *Of the different Quantities of Rain, which appear to fall, at different Heights, over the same Spot of Ground.* By William Heberden, *M. D. F. R. S.*

Read Dec. 7, 1769.

A COMPARISON having been made between the quantity of rain, which fell in two places in London, about a mile diftant from one another, it was found, that the rain in one of them conftantly exceeded that in the other, not only every month, but almoft every time that it rained. The apparatus ufed in each of them was very exact, both being made by the fame artift; and upon examining every probable caufe, this unexpected variation did not appear to be owing to any miftake, but to the conftant effect of fome circumftance, which not being fuppofed to be of any moment, had never been attended to. The rain-gage in one of thefe places was fixed fo high, as to rife above all the neighbouring chimnies; the other was confiderably below them; and there appeared reafon to believe, that the difference of the quantity of rain in thefe two places was owing to this difference in the placing of the veffel in which it was received. A funnel was therefore placed above the higheft chimnies,

chimnies, and another upon the ground of the garden belonging to the ſame houſe, and there was found the ſame difference between theſe two, though placed ſo near one another, which there had been between them, when placed at ſimilar heights in different parts of the town. After this fact was ſufficiently aſcertained, it was thought proper to try, whether the difference would be greater at a much greater height; and a rain-gage was therefore placed upon the ſquare part of the roof of Weſtminſter Abbey, being at ſuch a diſtance from the weſtern towers, as probably to be very little affected by them, and being much higher than any other neighbouring buildings. Here the quantity of rain was obſerved for a twelvemonth, the rain being meaſured at the end of every month, and care being taken, that none ſhould evaporate, by paſſing a very long tube of the funnel into a bottle through a cork, to which it was exactly fitted. The tube went down very near to the bottom of the bottle, and therefore the rain, which fell into it, would ſoon riſe above the end of the tube, ſo that the water was no where open to the air except for the ſmall ſpace of the area of the tube: and by trial it was found, that there was no ſenſible evaporation through the tube thus fitted up.

The following table will ſhew the reſult of theſe obſervations.

From

From July the 7th, 1766, to July the 7th, 1767, there fell into a rain-gage fixed

	Below the top of a houſe.	Upon the top of a houſe.	Upon Weſtminſter Abbey.
	inch.	inch.	inch.
1766 from the 7th of July to the end	3,591	3,210	2,311
Auguſt	0,558	0,479	0,508
September	0,421	0,344	
October	2,364	2,061	1,416
November	1,079	0,842	0,632
December	1,612	1,258	0,994
1767 January	2,071	1,455	1,035
February	2,864	2,494	1,335
March	1,807	1,303	0,587
April	1,437	1,213	0,994
May	2,432	1,745	1,142
June	1,977	1,426	1,145
from the 1ſt of July to the 7th	0,395	0,309	
	22,608	18,139	12,099

By this table it appears, that there fell below the top of a houſe above a fifth part more rain, than what fell in the ſame ſpace above the top of the ſame houſe, and that there fell upon Weſtminſter Abbey not much above one half of what was found to fall in the ſame ſpace below the tops of the houſes. This experiment has been repeated in other places with the ſame event. What may be the cauſe of this extraordinary difference has not yet been diſcovered; but it may be uſeful to give notice of it, in order to prevent that error, which would frequently be committed in comparing the rain of two places without attending to this circumſtance.

It is probable, that ſome hitherto unknown property of electricity is concerned in this phænomenon. This

 power

power has undoubtedly a great ſhare in the deſcent of rain, which hardly ever happens, if the air and electrical apparatus be ſufficiently dry, without manifeſt ſigns of electricity in the air. Hence it is, that in Lima, where there is no rain, they never have any lightning or thunder (*a*); and that, as M. Tournefort was aſſured, it never rains in the Levant but in winter, and that this is the only ſeaſon in which any thunder is heard (*b*). If this appearance therefore could be accounted for, it would probably help us to ſome more ſatisfactory cauſes of the ſuſpenſion of the clouds, and of the deſcent of rain.

(*a*) See the Engliſh tranſlation of the voyage of Don George Juan and Don Antonio de Ulloa to South America, vol. II. book i. chap. 6. p. 69 and 79.

(*b*) Voyage du Levant, let. X. p. 429.

XLVIII. *An Account of an Observation of an Eclipse of the Moon, made at* Hawkhill, *near* Edinburgh. *In a Letter to the Astronomer Royal, from* James Lind, *M. D.*

Edinburgh, December 14, 1769.

DEAR SIR,

Read Dec. 21, 1769.

I HERE send you our observations on the eclipse of the Moon, the 12th current; I wish it had rather been an account of an occultation, but the seeing of them seems to be denied to us: the night of the last was the only cloudy night that has been here for these four weeks past; the weather having been more like summer weather than that of winter.

The morning of the eclipse was very clear, and inclining to frost. Before we got to the observatory, near one third of the Moon's disc, where the first contact began, was covered with a smoaky appearance, which made us apprehend the eclipse was begun; but, on getting to the observatory, we saw, by our telescopes, the Moon's limb was still untouched;

about five minutes after, at 16^h 30′ 51″ mean time, a thick darkneſs came on the Moon's upper limb, which was ſtill diſtinctly to be ſeen through it. At 16^h 39′ 21″ the limb was broke. The middle of the eclipſe was reckoned to be the middle time betwixt the diſappearing of this limb, and the re-appearing of the other. I likewiſe ſend you the ſidereal times, by which the obſervations were taken, each obſervation being corrected to leſs than half a ſecond; the mean times are not ſo near, being taken from the other by means of a table; but are ſtill correct enough for obſervations which, of themſelves, cannot be obſerved with great preciſion.

ECLIPSE OF THE MOON,

December 12, 1769, at Hawkhill.

	Sid. time.				Mean time.		
	h	′	″		h	′	″
1ſt contact of darkneſs	9	59	19	=	16	30	51
Moon's limb broke	10	7	50	=	16	39	21
Clear ſpot in or about Terra Pruinæ touched	10	15	22	=	16	46	51
Ditto diſappeared	10	16	02	=	16	47	31
Copernicus touched	10	28	58	=	17	0	25
Copernicus central	10	30	32	=	17	1	59
Copernicus diſappeared	10	31	32	=	17	2	59
Mare Criſium touched	10	59	53	=	17	31	15
Ditto diſappeared	11	9	23	=	17	40	44
Middle of eclipſe	11	32	37	=	18	3	54

1ſt

	Sid. time.				Mean time.		
	h	′	″		h	′	″
1ſt clear ſpot re-appeared, but indiſtinctly, being a little cloudy	12	14	24	=	18	45	34
Mare Criſium totally emerged	12	47	10	=	19	18	29
Moon's limb compleated	12	57	24	=	19	28	27
2d contact of darkneſs	12	59	24	=	19	30	27
End of ſmoaky appearance	13	5	25	=	19	38	27

Towards the end it was very clear, and I make no doubt but we ſhould have ſeen the ſmoaky appearance, had it not been for the day-light coming on.

Since I wrote to you laſt, we have taken another meridian obſervation for the latitude, and made it 55° 57′ 30″ N.

In looking into the Abridgment of the Philoſophical Tranſactions, Vol. VII. p. 140, I found an account of an annular eclipſe of the Sun, obſerved at Edinburgh, by Mr. Mac Laurin, in the year 1737; which may help to determine our longitude for the preſent, till it is done more correctly by occultations of the ſtars by the Moon, or eclipſes of Jupiter's ſatellites. I am, with reſpect,

SIR,

Your moſt devoted, humble ſervant,

James Lind.

REMARKS.

Remarks by the Astronomer Royal.

The beginning of the eclipſe was obſerved at the Royal Obſervatory at $10^h\ 20'\ 29''$, and the biſſection of Copernicus at $10^h\ 43'\ 23''$ ſidereal time; which, compared with the correſpondent obſervations above, give $12'\ 39''$ and $12'\ 51''$ of time, for the difference of meridians of Hawkhill and Greenwich.

Nevil Maſkelyne.

Received October 5, 1769.

XLIX. *An Account of Two* Auroræ Boreales *observed at* Oxford. *In Two Letters to* Mathew Maty, *M. D. Sec. R. S.* *from the Rev.* John Swinton, *B. D. F. R. S.* *Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

Good Sir,

Read Dec. 7, 1769.

CASTING my eye towards the northern part of the hemisphere, on Sunday, February 26, 1769, about 8h. 30′ P. M. I discovered there a pretty bright *Aurora Borealis* of the common kind. For a short time, there was a very quick succession of lucid columns, and coruscations; which seemed smaller than they usually are in such meteors, that appear often enough here. They were of so pale a yellow colour, that some of them seemed to be almost perfectly white. In this, however, nothing very remarkable, or uncommon, could be discerned.

But

But what principally engaged my attention, at this time, was the gradual approach of the *Aurora* towards the ſouth, inſomuch that though it was at firſt moſt apparently an *Aurora Borealis*, and that of the common kind; it nevertheleſs, by the gradual variation of its original poſition, ſeemed to have commenced a ſort of *Aurora Auſtralis*, (1) of which uncommon ſpecies of meteors I have given a ſhort account in one of my former papers, before its extinction. This happened a little before nine o'clock; after which nothing worthy of notice, as far as I could learn, during the remainder of the night, in any part of the heavens, occurred.

As the gradual variation of the original poſition, or ſituation, of this phænomenon ſeemed to me a pretty extraordinary circumſtance, and was ſuch a one as I had never obſerved, nor heard of, before; I thought myſelf hereby ſufficiently authorized to communicate the very conciſe deſcription of this meteor, now ſent you, to the Royal Society. You will therefore be ſo good as to excuſe the trouble given you by the communication; and believe me to be, with all poſſible conſideration and eſteem,

SIR,

Your moſt obedient humble ſervant,

Chriſt-Church, Oxon.
Sept. 28, 1769.

John Swinton.

(1) *Philoſoph. Tranſact.* vol. LIV. for the year 1764, p. 328, 329. Lond. 1765.

LETTER

Received October 5, 1769.

LETTER II.

GOOD SIR,

Read Dec. 7, 1769.

BEING in my parlour, with the sashes down, on Saturday, September 9, 1769, at 8^h $20'$ P. M. I observed, with no small degree of astonishment, through the glass, such a redness in the sky as proceeds from the reflection of a great fire. This I was at first inclined to consider as a sort of deception, occasioned by the glass through which so uncommon an object seemed to present itself to my view; but stepping out immediately into the yard, I found it to be a real appearance, resembling a flame, in the atmosphere, and consequently a very unusual sight. The meteor was, however, of no very considerable extent; being almost intirely confined to that small tract of the heavens occupied by *Ursa Major*, part of *Ursa Minor*, and the intermediate space, containing the tail of *Draco*, between those two constellations. It remained about $20'$, after I first discovered it, without any material change, or variation; and at 8^h $40'$ P. M. as I found by consulting my watch, almost instantly disappeared.

The wind on the 9th was, for the most part, W. and S. W. and the weather showery. The rain, however,

however, notwithſtanding the favourable ſituation of the wind, was ſomewhat cold, and the whole day had a lowering winterly aſpect. A ſmall ſhower fell, juſt before I diſcovered the phænomenon here deſcribed. The light caſt by it was nearly equal to that of the full moon, on a cloudy night. The 10th the wind continued in the ſame quarter as before; and the weather was much the ſame, attended by a diſagreeable chillneſs in the air, as that of the preceding day. All the principal ſtars of the abovementioned conſtellations very clearly and diſtinctly appeared, through the ſeemingly fiery vapour, with which the tract occupied by them was ſo ſtrangely and ſo remarkably tinged.

As the luminous appearance ſeen at London, between eight and nine o'clock, the ſame night, from the ſhort account given of it in one of the public (1) papers, ſeems to have agreed in all reſpects with that obſerved by me at Oxford, at the very ſame time, it may be conſidered, without any impropriety, as the very meteor here deſcribed. Admit this, and I can ſee nothing improbable or unnatural in ſuch a ſuppoſition, and it muſt be allowed, that the atmoſphere was at London in the ſame diſpoſition, with regard to the exhibition of this ſpecies of meteors, as at Oxford, the very ſame inſtant of time; and impregnated in both places with the ſame kind of luminous vapour, at that inſtant, which occaſioned the production of the phænomenon I have here been endeavouring to deſcribe.

(1) *The Gazetteer and Evening Advertiſer*. Nº 1265. SEPTEMBER 11, 1769.

It

It may not be improper to obſerve, that the luminous appearance of September 9, 1769, in ſeveral reſpects, was ſimilar to that moſt remarkable one ſeen by me here, December 5, 1737; but differed from it in the three following particulars: 1. The former of theſe was not ſo red, nor did ſo much reſemble the colour of blood, as the latter. 2. The former did not tinge near ſo conſiderable a part of the hemiſphere as the latter. 3. The meteor of December 5, 1737, which I perfectly well remember, was of a much longer duration, than that of September 9. 1769. All which will very clearly appear from Dr. Huxham's ſhort deſcription of the former of theſe meteors, tranſmitted to the Royal Society, and printed in the (2) Philoſophical Tranſactions, compared with the ſhort account of the latter, drawn up by me, in the preceding part of this paper. The meteor of December 5, 1737, different from moſt, if not all the others, that had till that time been obſerved, was looked upon, if I remember right, as a ſingular appearance, by the great Dr. Halley himſelf. For, that moſt excellent aſtronomer, mathematician, and phyſiologiſt, I think, told me, when we dined together, at Mr. Swete's, in Greenwich, the following year, that he had never met with a ſimilar phænomenon, in the whole courſe of his obſervations.

I muſt not forget to relate, that a moſt tranſcendent brightneſs, or very uncommon illuſtration of the atmoſphere in the north, preſented itſelf to my view, on September 9, 1769, at 10^h $15'$ P. M. which

(2) *Philoſoph. Tranſact.* vol. XL. for the month of *December*, 1738, p. 437, 438.

 covered

covered about half of the interjacent ſpace between *Urſa Major* and the horizon. The light caſt by this moſt remarkable *crepuſculum* ſeemed much to exceed that of the full moon. It was not, however, of any long continuance; the whole being abſorbed by dark fuſcous clouds, in leſs than a quarter of an hour after I firſt perceived it. This I could not help conſidering as a ſort of ſequel to the unuſual phænomenon that had preſented itſelf to my view about two hours before.'

Since I began this paper, I have been informed by Mr. Parſons, ſtudent of Chriſt-Church, that he and the Reverend Mr. Whitchurch, likewiſe ſtudent of Chriſt-Church, on Thurſday, September 21, 1769, between 8^h and 9^h P. M. diſcovered a dark and blackiſh cloud, or vapour, in the north, contiguous to the horizon; from whence iſſued a meteor, which bore a much greater reſemblance to that of December 5, 1737, both in colour and extent, than the luminous appearance I had obſerved twelve days before. The colour of this meteor was ſo vivid, and the whole formed ſo lively a repreſentation of a great deep red flame, that thoſe gentlemen, as well as others that ſaw it, imagined ſome of the houſes in the town to be ſet on fire; but, upon inquiry, found that no ſuch accident had happened. It tinged a conſiderable tract in the northern part of the hemiſphere, and particularly the ſpace between *Urſa Major* and the horizon. It ſoon grew very pale, and its total extinction was ſo fully completed by 8^h $45'$ P. M. that not the fainteſt traces of it could then be diſcerned.

As very few, if any, inſtances of this ſpecies of meteors, which greatly differ from the common *Auroræ Boreales*, have occurred ſince Dr. Halley's death, which happened in the beginning of 1742; and as I am fully perſuaded of his having declared to me at Greenwich, as has been already obſerved, that he had met with only one of them, in the whole courſe of his obſervations; I was inclined to believe, that the ſhort account of another, or rather two others, of them, tranſmitted to you in this paper, would not prove unacceptable to the Royal Society, and therefore that you would excuſe the trouble given on this occaſion, by,

GOOD SIR,

Your much obliged,

and moſt obedient,

humble ſervant,

Chriſt-Church, Oxon.
Sept. 30, 1769.

John Swinton.

P. S. October 2, 1769. We had laſt night here ſome beautiful *Auroræ Boreales*, of the common kind; which began to appear in the north and north-eaſt, a little after 8^h, and continued till near 9^h P. M. The flaſhes, or coruſcations, were of a very pale yellow colour, and ſome of them aſcended up to the zenith; the undulations of ſhining matter were quick and numerous, and the city almoſt as light as if illuminations had been made in ſeveral parts of it.

L. *Obſervations of the Tranſit of* Venus *on* June 3, 1769, *and the Eclipſe of the* Sun *on the following Day, made at* Paris, *and other Places. Extracted from Letters addreſſed from M.* De la Lande, *of the Royal Academy of Sciences at* Paris, *and F. R. S. to the Aſtronomer Royal; and from a Letter addreſſed from M.* Meſſier *to Mr.* Magalhaens.

Read Dec. 14, 1769.

M. MESSIER, with the beſt achromatic teleſcope at Paris, of 12 feet focus, made by M. Antheaulme, obſerved the firſt internal contact at $7^h\ 38'\ 43''$, apparent time reduced to the Royal Obſervatory, and, he thinks, without an uncertainty of two ſeconds: and this is the obſervation in which I moſt confide. M. Du Sejour, and M. Caſſini, the ſon, at the Royal Obſervatory, with much leſs teleſcopes, obſerved it alſo at $7^h\ 38'\ 43''$. M. de Fouchy, M. Bailly, M. De Borry, and two opticians, who were at the *Meute*, obſerved the contact at $7^h\ 38'\ 45''$, reduced to Paris. M. Caſſini de Thury, at the Royal Obſervatory, noted it at $7^h\ 38'\ 53''$; M. the Duke de Chaulnes at $7^h\ 38'\ 57''$; both with new achromatic teleſcopes of Dollond of $3\frac{1}{2}$ feet. M. Maraldi, at the Royal Obſervatory,

ſervatory, obſerved at $7^h\ 38'\ 50''$, with a good achromatic teleſcope of 3 feet, made at Paris, but he thinks the obſervation liable to an error of ten ſeconds. M. Le Mannier, at St. Hubert, obſerved at $7^h\ 38'\ 51''$, reduced to Paris (by adding $1'\ 58''$). M. Fouguere, at Bourdeaux, at $7^h\ 38'\ 50''\frac{1}{2}$, reduced to Paris, taking in the difference of parallax, which is two ſeconds greater at Paris than Bourdeaux.

We have alſo received ſeveral obſervations of the eclipſe of the Sun. M. de Thury ſaw the beginning at $6^h\ 46'\ 49''$ apparent time. M. Jeaurat, at $6^h\ 46'\ 40''$, at the Military School, which is $7''\frac{1}{2}$ to the weſt of the Obſervatory. M. Maraldi ſaw the end at $8^h\ 27'\ 11''$. M. Jeaurat at $8^h\ 27'\ 4''$, or $8^h\ 27'\ 11''\frac{1}{2}$ reduced to the Obſervatory. M. Meſſier at $8^h\ 27'\ 24''$.

The obſervers at Rochfort, Lyons, and Avignon, did not ſee the tranſit of Venus; it was obſerved at Breſt at $7^h\ 12'\ 5''$, or forty ſeconds later than at Paris, if we ſuppoſe the difference of the meridians to be well known. This point we ſhall examine hereafter. For my part, I could not obſerve the internal contact of Venus; I was preciſely in the place where the clouds came on twenty-five ſeconds too ſoon; neither was it obſerved at the Military School, which is cloſe to Paris.

There was alſo another obſerver at Breſt, M. Verdun, an officer of the marine, who obſerved the internal contact of Venus at $7^h\ 11'\ 37''$, apparent time, which makes $7^h\ 38'\ 58''$, reduced to Paris. The end of the Solar eclipſe was obſerved at Breſt at $7^h\ 56'\ 33''$ and $7^h\ 56'\ 44''$ by the different obſervers.

M. Pingré,

M. Pingré, at Cape Francois, obſerved the two contacts of Venus in the latitude of 19° 47′ at 2^h 26′ 12″, and 2^h 44′ 44″, apparent time, with a five feet achromatic teleſcope, but we do not yet know the longitude of the place ſufficiently. I expect him to return ſoon, and that he will himſelf draw the concluſions from his obſervations. At Martinico, one of our miſſionaries obſerved the contacts at 3^h 15′ 14″, and 3^h 33′ 57″; when we have the longitude exact, this obſervation will alſo be of uſe.

I will ſend you ſhortly all the obſervations of the 1ſt ſatellite, which I can collect. Here are ſome made at Gottingen, which I have juſt received from Mr. Liunberg.

				h	′	″	
1769	April 21	Im. 1 Sat.		13	21	37	Refractor of 14 feet.
	28			15	14	34	Ditto of 10 f. of Liberkuhhn.
	30			9	43	21	The ſame.
	May 23	Em. 1		12	6	31	

So far M. De Lalande.

The Extract of M. Meſſier's Letter to M. Magalhaens is as follows:

I obſerved the tranſit of Venus, June 3, 1769, at the College of Louis le Grand at Paris, which is 2″ to the eaſt of the meridian of the Royal Obſervatory. I had an achromatic teleſcope, of 12 feet focus, which had an aperture of $3\frac{3}{4}$ inches, and magnified 180 times, with the view of making my obſervation more correſpondent to that which M. the Abbé Chappe was to make, in California, with a teleſcope of the ſame length, the ſame magnifying power, and equal goodneſs. The firſt contact could not be ſeen, on account of a very thick cloud; there even fell ſome rain.

rain. I waited for the ſecond; the Sun then was pretty clear. But there were ſome vapours, which cauſed ſuch great undulations as to hinder me from ſeeing the diſc of the Sun, and that of Venus, well defined. At $7^h\ 38'\ 45''$, apparent time, or $7^h\ 38'\ 43''$, reduced to the meridian of the Royal Obſervatory, the ſecond contact appeared deciſively to me. Two ſeconds after, a very fine thread of light appeared between the limb of Venus and that of the Sun; ſo that in my obſervation there is not an uncertainty of two ſeconds in the moment of the internal contact. After this obſervation, I viewed the Sun with different glaſſes, which rendered him alternately red and white. I ſaw Venus, with this laſt colour, with a creſcent of a blueiſh colour; and a little inclined towards the limb of the Sun: with the glaſs which made the image of the Sun red this creſcent diſappeared; but I ſaw Venus flattened in the direction of the creſcent. I meaſured the greateſt and leaſt diameter; the greateſt was $56''\frac{1}{2}$, and the leaſt $53''\frac{1}{2}$. Perhaps this creſcent was only viſible by the effect of ſome optic illuſion; but I relate only what I ſaw. At $7^h\ 52'\ 8''$, apparent time, I meaſured the interval between the limb of Venus and that of the Sun, which I found $46''\frac{1}{2}$; and at $7^h\ 58'\ 4''$, the firſt limb of Venus touched the horizon.

By a letter from M. l'Abbé Bourriot to Mr. Magalhaens it appears, that Meſſ. de Fouchy and Bailly, at the Meute, each made uſe of reflecting teleſcopes of 30 inches focus and $4\frac{1}{2}$ inches aperture; that M. Bory made uſe of an achromatic teleſcope of 5 feet focus, and 2 inches aperture; and M. l'Abbé Bourriot made uſe of a very good achromatic teleſcope of 6 feet long, and $2\frac{1}{4}$ inches aperture, made by himſelf, magnifying 120 times.

Read December 14, 1769.

LI. *Transit of* Venus *over the* Sun, *observed* June 3, 1769, *by* Alexander Aubert, *in* Austin Friars, London, *three Seconds of Time East of* St. Paul's, *with a Cassegrain Reflector of* J. Short, *having a Metal of two Feet focal Length, and magnifying about* 110 *Times.*

	h	′	″	
External contact at	7	8	13	mean time.
Internal contact at	7	26	45	interval 18′ 32″.

N.B. At $7^h\ 26'\ 45''$ Venus appeared to me in contact with the Sun, and about 6″ after I saw the Sun's limb compleated.

The clock could be depended on to less than one second, having been compared with a number of equal altitudes of the Sun, some days before and after the transit.

Alexander Aubert.

LII. *Some*

Received October 31, 1769.

LII. *Some Account of an Oil, transmitted by Mr.* George Brownrigg, *of* North Carolina. *By* William Watson, *M. D. R. S. S.*

To the ROYAL SOCIETY.

GENTLEMEN,

Read Dec. 14, 1769.

THE application of natural productions to the benefit of mankind, has always been an object of our excellent institution; and endeavours to extend the utility of substances already very obscurely known, have always met from you a favourable reception.

It is with this view, that I lay before you some pods of a vegetable, and the oil pressed from their contents. They were sent from Edenton, in North Carolina, by Mr. George Brownrigg, whose brother, Dr. Brownrigg, is a worthy member of our Society; and are the produce of a plant well known, and much cultivated, in the Southern colonies, and in our American sugar islands, where they are called ground

 nuts,

nuts, or ground peafe. They are originally, it is prefumed, of the growth of Africa, and brought from thence by the negroes, who ufe them as food, both raw and roafted, and are very fond of them. They are therefore cultivated by them in the little parcels of land fet apart for their ufe by their mafters. By thefe means, this plant has extended itfelf, not only to our warmer American fettlements, but it is cultivated in Surinam, Brafil, and Peru.

The plant, which produces thefe, has been mentioned, and defcribed, by the botanical writers of the later times. Ray, in his Hiftory of Plants, calls it *Arachis Hypogaios Americanus*. It is the *Arachidna quadrifolia villofa* of Plumier. Sir Hans Sloane, in his Hiftory of Jamaica, calls it *Arachidna Indiæ utriufque tetraphylla*. Pifo and Marcgraac both mention it among the Brafilean plants, under the name of *Mundubi*. Linnæus has conftituted a genus of this plant, of which only one fpecies is as yet known, under Mr. Ray's generical name of *Arachis*.

This plant, together with a very few of the trifoliate tribe, has the property of burying its feeds under ground, which it does in the following manner. As foon as the plant is in flower, its flower is bent towards the ground until it touches it. The pointal of the flower is then thruft into the ground to a fufficient depth, where it extends itfelf, and forms the feed-veffel and fruit, which is brought to maturity under ground, from whence it is dug up for ufe.

This plant, which is a native of warm climates, will not bear being cultivated to advantage in Great-Britain, or in the northern colonies; but, according to Mr. Brownrigg, in fouthern climates its produce

is

is prodigious; and what adds to its value is, that rich land is not neceſſary for its cultivation, as light ſandy land, of ſmall value, will produce vaſt crops of it. Beſides what the negroes cultivate for their own uſe, ſome planters raiſe a conſiderable quantity of it, for the feeding of ſwine and poultry, which are very fond of the ground peaſe; and, when they are permitted to eat freely of them, ſoon become fat.

Mr. Brownrigg, from whom, as I before mentioned, I received the oil, conſiders the expreſſing oil from the ground peaſe, as a diſcovery of his own: it may, perhaps, at this time, be very little practiſed either in North Carolina, the place of his reſidence, or elſewhere. But certain it is, that this oil was expreſſed above fourſcore years ago; as Sir Hans Sloane mentions it, in the firſt volume of his Hiſtory of Jamaica; and ſays, that this oil is as good as that of almonds. It is probable, however, that ſmall quantities only were expreſſed, and that even at that time the knowledge of it did not extend very far. Mr. Brownrigg therefore is highly praiſe-worthy in reviving the remembrance of procuring oil from theſe ſeeds. It is obtained, by firſt bruiſing the ſeeds very well, and afterwards preſſing them in canvas bags, as is uſual in procuring oil from almonds or linſeed.

To have the oil in the beſt manner, no heat ſhould be uſed. The heating the cheeks of the preſs increaſes the quantity of the oil, but leſſens its goodneſs, where it may be intended to be uſed as food, or as a medicine. For other purpoſes, the larger quantity of oil, obtained by heat, will anſwer equally well.

Neither the ſeeds nor oil are apt to become rancid by keeping; and as a proof of this, the oil before you,

which was ſent from Carolina in April laſt; and, without any particular care, has undergone the heats of laſt ſummer, is yet perfectly ſweet and good. Theſe ſeeds furniſh a pure, clear, well taſted oil; and, as far as appears to me, may be uſed for the ſame purpoſes, both in food and phyſic, as the oils of olives or almonds. It may be applied likewiſe to many, if not all, the œconomical purpoſes with the former of theſe.

But what greatly adds to the merit of what Mr. Brownrigg has informed us of, is the low price, at which this oil may be obtained. He ſays, that ten gallons of the peaſe, with the huſks unſhelled, will, without heat, yield one gallon of oil; if preſſed with heat, they will afford a much larger quantity. The value of a buſhel of theſe, in Carolina, does not exceed, as I have been informed, eight pence, or thereabouts. Theſe will furniſh a gallon of oil, the labour and apparatus to procure which, cannot coſt much. This price will not amount to ſo much as a fourth of what the beſt Florence oil of olives coſts in England. This therefore ought to be conſidered as valuable information, as, on account of its cheapneſs, a larger portion of mankind than at preſent may be permitted to uſe oil with their food, from whom it is now withheld on account of its price.

Great quantities of olive oil are ſent from Europe to America. New England alone, Mr. Brownrigg ſays, annually conſumes twenty thouſand gallons. The quantities uſed in his majeſty's other dominions in America muſt be prodigious. The oil from ground peaſe, of which any quantity deſired may be raiſed, may and would ſupply this conſumption of olive oil. It would likewiſe, I am perſuaded, bear exportation

to any of thoſe places where the oil of olives is uſually carried; and thereby become a valuable article of commerce.

After the oil has been expreſſed from the ground peaſe, they are yet excellent food for ſwine.

Preſuming that a more intimate knowledge of the vegetable production before you, than what we were lately poſſeſſed of, would not be diſagreeable to the Royal Society, I take the liberty of laying the preſent account before you; and am,

GENTLEMEN,

Your moſt obedient,

humble ſervant,

William Watſon.

LIII. *A Catalogue of the Fifty Plants from* Chelsea *Garden, presented to the* Royal Society *by the worshipful Company of Apothecaries, for the Year* 1768, *pursuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præses. By* William Hudson, *Societatis Regiæ et clariss. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelsean. *Præfectus et Prælector Botanicus.*

Read Dec. 14, 1769.

2301 ACONITUM *uncinatum*, foliis multilobis, corollarum galea apice basi longius producto. Lin. Spec. plant. 750.

2302 Adiantum *Capillus-Veneris*, frondibus decompositis, foliolis alternis, pinnis cuneiformibus, lobatis pedicellatis. Lin. Spec. plant. 1558. Hudf. fl. Angl. 391.
Adiantum foliis coriandri. Bauh. pinn. 356.

2303 Andropogon, *Ischæmum*, spicis digitatis plurimis, flosculis sessilibus, aristato muticoque, pedicellis lanatis. Lin. Spec. plant. 1483.
Gramen dactylon spicis villosis. Bauh. pin. 8.

2304 An-

2304 Anthemis *maritima*, foliis pinnatis dentatis carnosis nudis punctatis, caule prostrato, calycibus tomentosis. Lin. Spec. plant. 1259.

Matricaria maritima. Bauh. pin. 134.

2305 Anthemis *mixta*, foliis simplicibus dentato-laciniatis. Lin. Spec. plant. 1260.

Chamæmelum annuum ramosum coronopi folio, flore mixto. Hist. Ox. III. 36. tab. 18. fig. 15.

2306 Anthyllis *coccinea*, herbacea, foliis pinnatis inæqualibus, capitulo duplicato. Lin. Spec. plant. 1012. Hudf. fl. Angl. 273.

Vulneraria supina, flore coccineo. Dill. Hort. Elth. 431. tab. 320. fig. 413.

2307 Asclepias *curassavica*, foliis lanceolatis petiolatis glabris, caule simplici, umbellis erectis solitariis. Lin. Spec. plant. 314.

Apocynum radice fibrosa, petalis coccineis, corniculis croceis. Dill. Hort. Elth. 34. tab. 30. fig. 33.

2308 Ascyrum *hypericoides*, foliis oblongis, ramis ancipitibus. Lin. Spec. plant.

Hypericum pumilum semper virens, caule compresso ligneo, ad bina latera alato, flore luteo tetrapetalo. Pluk. plant. 104. R. Hist. 111. 495.

2309 Asplenium, *ruta muraria*, frondibus alternatim decompositis, foliolis cuneiformibus crenulatis. Lin. Spec. plant. 1541.

2310 Ballota *alba*, foliis cordatis indivisis serratis, calycibus subtruncatis. Lin. Spec. plant. 814.

Ballote flore albo. Tourn. Inst. 185.

2311 Bidens *bullata*, foliis ovatis serratis, inferioribus oppositis, superioribus ternatis, intermedio majore. Lin. Spec. plant. 1167.

2312 Cassytha *filiformis*. Asb. it. 243. Lin. Spec. plant. 530.

Cuscuta baccifera Barbadensium. Pluk. phyt. tab. 172. f. 2.

2313 Cenchrus *racemosus*, panicula spicata, glumis muricatis, setis ciliaribus. Lin. Spec. plant. 1487.

Gramen caninum maritimum asperum. Bauh. prodr. 2. tab. 2.

2314 Chrysanthemum *corymbosum*, foliis pinnatis, inciso-serratis, caule multifloro. Roy Lugdb. 174. Lin. Spec. plant. 1251.

Tanacetum montanum inodorum, minore flore, Bauh. pin. 132.

2315 Cistus *salicifolius*, herbaceus, patulus, villosus, stipulatus, floribus racemosis erectis, pedicellis horizontalibus. Lin. Spec. plant. 742.

Cistus folio salicis. Bauh. pin. 464.

2316 Elymus *virginicus*, spica erecta spiculis trifloris, involucro striato. Lin. Syst. Nat. 101. Spec. plant. 123.

2317 Elymus *hystrix*, spica erecta spiculis involucro destitutis patentibus. Lin. Spec. plant. 124.

2318 Erica *cinerea*, antheris bicornis inclusis, corollis ovatis racemosis, foliis ternis glabris linearibus. Lin. Spec. plant. 501. Hudf. Angl. 144.

Erica

Erica humilis, cortice cinereo, arbuti flore. Bauh. pin. 486.

2319 Erigeron *graveolens*, ramis lateralibus multifloris, foliis lanceolatis integerrimis, calycibus squarrosis. Lin. Spec. plant. 1219.

2320 Fumaria *capreolata*, pericarpiis monospermis racemosis, foliis scandentibus subcerrhosis. Lin. Spec. plant. 985.

Fumaria major scandens flore pallidiore. Raj. Syn. 204.

2321 Heliotropium *Indicum*, foliis cordato-ovatis acutis scabriusculis, spicis solitariis, fructibus bifidis. Lin. Spec. plant. 187.

Heliotropium Americanum cæruleum. Pluk. phyt. tab. 245. f. 4.

2322 Hordeum *vulgare*; flosculis omnibus hermaphroditis, aristatis; ordinibus duobus erectioribus. Lin. Spec. plant. 125.

Hordeum polystichon vernum. Bauh. pin. 22. Hist. Ox. 111. s. 8. tab. 6. f. 3.

2323 Hordeum *cæleste*, flosculis omnibus hermaphroditis feminibus decorticatis. Lin. Spec. plant. 125.

2324 Hordeum *distichon*, flosculis lateralibus masculis muticis, feminibus angularibus imbricatis. Lin. Spec. plant. 125.

Hordeum distichon. Bauh. pin. 22. Hist. Ox. 111. f. 8. tab. 6. fig. 1.

2325 Horminum *virginicum*, foliis cuneiformi-oblongis, caule bifolio. Lin. Spec. plant. 832.

Melissa atrorubens bugulæ folio. Dill. Hort. Elth. 219. tab. 175. fig. 216.

2326 Justicia *hyssopifolia*, fruticosa, foliis lanceolatis integerrimis, pedunculis trifloris ancipitibus, bracteis calyce brevioribus. Lin. Spec. plant. 21.

2327 Lactuca *scariola*, foliis verticalibus carina aculeatis. Lin. Spec. plant. 1119.
Lactuca sylvestris costa spinosa. Bauh. pin. 123.

2328 Lathyrus, *Aphaca*, pedunculis unifloris, cirrhis aphyllis, stipulis sagitato-cordatis. Lin. Spec. plant. 1029.
Aphaca. Lob. ic. 2. p. 70.

2329 Lythrum, *Hyssopifolia*, foliis alternis linearibus, floribus hexandris. Lin. Spec. plant. 642.
Hyssopifolia. Bauh. pin. 218.

2330 Milium *paradoxum*, floribus paniculatis aristatis. Lin. Spec. plant. 90.
Gramen avenaceum paniculatum galloprovinciale, aquilegiæ semine. Hist. Ox. 111. 214. Pluk. phyt. tab. 32. f. 2.

2331 Osmunda *crispa*, frondibus supra decompositis, pinnis alternis subrotundis incisis. Lin. Spec. plant. 1522. Hudſ. Angl. 383.
Adiantum crispum alpinum. Raj. Syn. 126.

2332 Panicum *Italicum*, spica composita, spiculis glomeratis setis immixtis, pedunculis hirsutis. Lin. Spec. plant. 83.
Panicum Italicum s. panicula majore. Bauh. pin. 27.

2333 Panicum *glaucum*, spica tereti involucellis bifloris fasciculato-setosis, seminibus undulato-nervosis. Lin. Spec. plant. 83.
Panicum spica simplici, aristis aggregatis flosculo subjectis. Gron. virg. 134.

2334 Passerina *hirsuta*, foliis carnosis extus glabris, caulibus tomentosis. Lin. Spec. plant. 513.
Thymelæa tomentosa, foliis sedi minoris. Bauh. pin. 461.

2335 Potentilla *Norvegica*, foliis ternatis, caule dichotomo, pedunculis axillaribus. Lin. Spec. plant. 715.
Quinquefolium hirsutum luteum paucioribus laciniis. Loesel. Pruss. 218. tab. 70.

2336 Prunella *grandiflora*, foliis omnibus ovato-oblongis petiolatis. Lin. Spec. plant. 837.
Prunella cærulea, magno flore. Bauh. pin. 261.

2337 Psoralea *corylifolio*, foliis simplicibus ovatis. Lin. Spec. plant. 1075.
Loto affinis coryli folio. Pluk. phyt. tab. 96. f. 5.

2338 Rubia *peregrina*, foliis quaternis. Lin. Spec. plant. 159.

2339 Salvia *Hispanica*, foliis ovatis, petiolis utrinque mucronatis, spicis imbricatis, calycibus trifidis. Lin. Spec. plant. 37.
Sclarea Hispanica. Tabern. Hist. 764. ic. 374.

2340 Sanicula Marilandica, flosculis masculis pedunculatis, hermaphroditis sessilibus. Lin. Spec. plant. 339.

2341 Satureia *hortensis*, pedunculis bifloris. Lin. Spec plant. 795.
Satureia hortensis. Bauh. pin. 218.

2342 Saxifraga *stellaris*, foliis serratis, caule nudo ramoso, petalis acuminatis. Lin. Spec. plant. 572. Hudf. Angl. 156.

2343 Saxifraga *aizoides*, foliis caulinis lineari-ſubulatis ſparſis nudis inermibus, caulibus decumbentibus. Lin. Spec. plant. 576.

Sedum Alpinum, flore pallido. Bauh. pin. 284. Hiſt. Ox. III. 477. ſ. 12. tab. 6. f. 3.

2344 Senecio *hieracifolius*, corollis nudis, foliis amplexi caulibus laceris, caule herbaceo erecto. Lin. Spec. plant. 1215.

Senecio Americanus altiſſimus blattariæ ſ. hieracii folio. Hern. par. 226. tab. 226.

2345 Serapias *latifolia*, bulbis fibroſis, nectarii labio crenato, obtuſo, petalis æquali, fructibus pendulis. Hudſ. Angl. 341.

Serapias bulbis fibroſis, foliis ovatis amplexicaulibus, floribus pendulis. Lin. Syſt. Nat. vol. II. 593.

Helleborine altera atro-rubente flore. Raj. Syn. 383.

2346 Serapias *paluſtris*, bulbis fibroſis, nectarii labio obtuſo crenato, petalis æquali, fructibus pendulis. Hudſ. Angl. 341.

Serapias bulbis fibroſis, foliis enſiformibus, floribus pendulis. Lin. Syſt. Nat. vol. II. 593.

Helleborine paluſtris noſtras. Raj. Syn. 283.

2347 Serratulo *ſcarroſa*, foliis lanceolatis integerrimis, calycibus ſquarroſis pedunculatis obtuſis lateralibus. Lin. Spec. plant. 1147.

Eupatoria affinis Americana bulboſa, floribus ſcarioſis capitulis contextis. Pluk. phyt. tab. 177. f. 4.

2348 Sideritis ſcordioides, foliis lanceolatis ſubdentatis ſupra glabris, bracteis ovatis dentato-ſpinoſis calycibus æqualibus. Lin. Spec. plant. 803.

2349 Tanacetum *annuum*, foliis bipinnatifidis linearibus acutis corymbis tomentoſis. Lin. Spec. plant. 1184.

Abſinthium corymbiferum annuum. Tourn. 458.

2350 Viburnum *dentatum*, foliis ovatis dentato-ſerratis plicatis. Lin. Spec. plant. 384.

LIV. *A Deſcription of the Lymphatics of the Urethra and Neck of the Bladder.* By Henry Watſon, *Surgeon to the* Weſtminſter *Hoſpital, and F. R. S.*

Read Dec. 14, 1769.

THE valvular lymphatics, as a ſyſtem of veſſels, *ſui generis*, are allowed to have a very conſiderable office in the animal œconomy; but an office, ſubordinate to that of the blood-veſſels: at leaſt, they have been ſuppoſed by many of the phyſiologiſts, not of ſo much conſequence, in preſerving the health and life of the animal.

If we conſider, that an obſtructed *thoracic duct*, which is in fact but a large lymphatic, will deſtroy life as effectually as a ligature made upon the *aorta* itſelf; we muſt conclude the lymphatics to be veſſels of much greater importance than ſome have imagined: nearly of as much conſequence, in ſupporting and carrying on the animal functions, as the arteries and veins themſelves: for if an obſtruction of the *aorta*, or great artery, can produce a very quick, or ſudden, death; an *obſtructed thoracic duct* will as certainly lead to a tedious and lingering one.

The caſe of the obſtructed *duct*, though not indeed often ſeen, yet is every now and then to be met with. It is the one cauſe of a *maraſmus* not known, or not attended to: generally owing to an enlargement of the

the lymphatic glands that lie near to, and in contact with the duct: generally too attended with obstructions in the more external conglobate glands; therefore always to be suspected, where we have these appearances, accompanied with a gradual wasting of the solids. In children and young subjects we meet with proofs of this disease; a disease, which never could have been learnt, but from the dissection of morbid bodies.

The lymphatics are said to be the true, and only system of absorbing vessels. I will suppose they are; though perhaps this opinion may yet admit of some doubts: however, they certainly are the vessels that take up the watery *latex* from most parts of the body, and return it back to be again mixed with the blood. This free absorption of the lymph is the great security against suffocation, injurious pressure, and an obstructed circulation in every part of the animal.

Many valuable discoveries have lately been made, of the existence of these vessels in birds, fish, and *amphibii*. That most accurate and indefatigable anatomist, Dr. Hunter, has writ fully and explicitly upon the lymphatics in the human body; and yet, still it is to be wished, we knew more about them.

We have not been able to see their origins in any one instance; we have not traced them through the whole body, as we have done the blood-vessels. It is reasonable to suppose they abound universally; but it is doubtful whether in many parts they exist, or not; for the most eminent anatomists confess, there are many parts, in which, hitherto, they have not been able to discover them.

It may not therefore be unentertaining to this learned Society, who ſo ſtudiouſly promote every uſeful inquiry, not only to have a demonſtrative proof of the exiſtence of lymphatics in a part of the human body where they have not as yet been diſcovered; but alſo to have an opportunity of knowing that the true origin of theſe veſſels may eaſily be ſhewn.

As to their preciſe origin, it has indeed been conjectured, and very reaſonably, from experiments *à poſteriori*.

It has been ſuppoſed they ariſe from all the ſurfaces and cavities of the body; becauſe thin fluids and ſubtle particles will be taken up from ſuch cavities, or ſurfaces, and will be readily enough conveyed into the blood: but then it has never been ſhewn, that they do ariſe from any one ſuch ſurface or cavity.

Commonly, the lymphatics are never filled from their beginnings, or little orifices. When they have been injected, it has always been done by uſing ſome violence; either by cutting into them, burſting, or tearing them aſunder; ſo that the injection rather gets in ſome how at the ſide, and not at the extremity of the veſſel.

The lacteal veſſels perhaps cannot, at leaſt have never been, to my knowledge, injected from the cavity of the inteſtine in the dead body. It is preſumed, that, as the lymphatics are ſimilar to theſe in other reſpects, their origins muſt be alſo ſimilar: that if the orifices of the lacteals are too fine to be diſcovered, the mouths of the lymphatics are alſo too delicate to be traced out. But with regard to the lymphatics of the human urinary bladder, it is certainly otherwiſe. When the part is freſh and ſound,

we may, with a little trouble, blow into the mouths of theſe veſſels, ſmall as they are; or introduce a fine briſtle into them, if we have but a ſteady hand and a good eye. I have frequently done both, in the preſence of many witneſſes; ſo that, without uſing the knife or lancet, the leaſt force or violence, air may be thrown into the lymphatics from their very beginnings; and mercury may be made to paſs by the ſame orifices.

Though I have ſaid we may eaſily have an ocular demonſtration of the origin or mouth of the lymphatic, in this part of the human body, I muſt confeſs, it is not always we can have that ſatisfaction: no part is more frequently diſeaſed: inflammation ſolders up the mouths of theſe little veſſels; and it is not to be expected we can ſhew their orifices when the *urethra* is in ſuch a ſtate.

It will always require ſome dexterity to catch the opening of the lymphatic; but the briſtle, once fairly introduced, will generally paſs with great eaſe ſome way within the veſſel.

Here then we may ſatisfy ourſelves in what manner the lymphatics do actually begin from ſurfaces: and to thoſe who, without ever having ſeen the origin of a lymphatic, have nevertheleſs reaſoned ſo well, and ſo juſtly, upon this ſubject, it may perhaps afford ſome pleaſure and ſatisfaction to find their conjectures agreeing ſo perfectly with the ſtructure.

The ſituation of the lymphatics, in general, is ſuperficial; that is to ſay, they are moſtly to be ſeen upon ſurfaces; though there are ſome deeper ſeated ones, which accompany the blood veſſels. They have been

well defcribed by authors, as exceeding fine, tender, and tranfparent veffels, frequently joining into one another, and interfected by a number of very delicate membranous pouches or valves; fo that, having an injection thrown into them, they give the appearance of being full of little knots.

The lymphatics are apparent enough, when they unite and grow large; but from their exility, want of colour, and tranfparency, are very difficult to be difcovered before.

Owing to thefe circumftances it is, that their origins have never before been feen; and that in many parts of the body, where they are neverthelefs fuppofed to exift, they ftill lie unnoticed. Haller, after fpeaking of thefe veffels in many other parts of the body, goes on thus: "Quæ a pene veniunt mihi minus nota "funt, fed dicta Cowpero. Alia huc a veficulis fe-"minalibus tendunt, aut certe ab earum vicinia, aut a "veficæ urinariæ fede, aut ab ipfa demum vefica, quæ "quidem vafcula iterum fateor mihi nondum vifa "effe." So that Haller, who knows fo well the ftructure of the human body, knows nothing of thefe lymphatics of the bladder, or membranous portion of the *urethra*.

The lymphatics of the urinary human bladder and *urethra*, firft fhew themfelves on each fide the *verumontanum* or *caput gallinaginis*, and by very little orifices take their origin from the internal membrane that lines the *urethra* and bladder, on whofe furface they open.

In their natural ftate, they appear like fo many fine threads lying clofe together, but diverging afterwards, as

as they paſs over the proſtate gland and neck of the bladder, and inoſculating or communicating very frequently, they form a kind of network or embroidery. From hence they are continued through the cellular membrane behind the internal coat of the bladder, and ſeem to join with the lymphatics of the *veſiculæ ſeminales*, to be continued with them to the neighbouring glands, and ſo on to the thoracic duct.

I have not been able to find lymphatics in any other part of the *urethra*; indeed, this canal ſeems to be perfectly void of them till we come to its membranous portion, where we meet with theſe I have been deſcribing; and it may be remarked, that here they are placed in that part of the *urethra* where the greateſt quantity of moiſture is ſupplied. Very probably the ſealing up the mouths of theſe delicate veſſels, by frequent inflammation and induration, may give riſe to that obſtinate *ſtillicidium* which is ſeldom or ever cured; owing to an accumulation of thin fluids, with a faulty abſorption.

Theſe lymphatics of the *urethra* and bladder alſo point out the road, by which any ſubtle *virus* may paſs, with the lymph, directly into the maſs of blood, and contaminate the whole habit, without giving the leaſt appearance of any local diſorder.

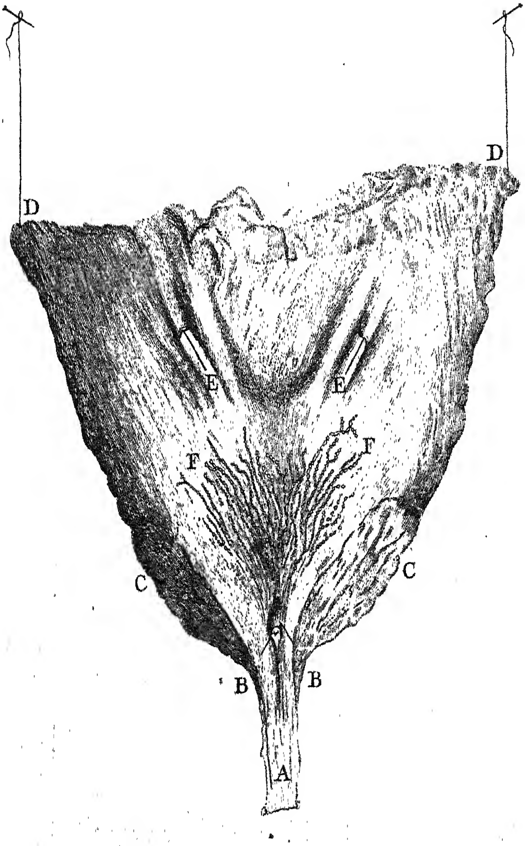
To have a clearer idea of the veſſels I have been treating of, I muſt beg leave to refer to the drawing annexed, in which theſe lymphatics of the urinary bladder and *urethra*, in the human body, are carefully and very accurately delineated.

Expla-

Explanation of TAB. XVI.

This drawing is an exact repreſentation of the lymphatics of the *urethra* and neck of the bladder, as they appear after having been injected with mercury, and preſerved in ſpirits.

A. The membranous portion of the *urethra* ſlit open.
B B. Briſtles in the ducts from the *veſiculæ ſeminales*.
C C. Proſtate gland.
D D. The inferior part of the bladder laid open.
E E. Briſtles in the *ureters* where they open into the bladder.
F F. The lymphatics.



J. Mynde sculp.

Read December 21, 1769.

LV. *Eclipses of* Jupiter's *First* Satellite, *the Eclipse of the* Moon, *and Occultations of* Fixed Stars *by the* Moon. *Observed at the Royal Observatory at* Greenwich, *in the Year* 1769. *Communicated by the Astronomer Royal.*

1769			Apparent time. h ′ ″	
March	29	Im.	12 25 7	with a 2 feet reflector of Short's constr.
April	12	Im.	16 16 8	with a 2 feet reflector of Bird's constr.
April	28	Im.	14 35 17½	with a 2 feet reflector of Short's.
May	16	Em.	9 32 15	with a 2 feet reflector of Short's.
			9 31 35	with a 6 feet reflector of Short's.
June	8	Em.	9 40 56	with a 6 feet reflector of Short's.
June	15	Em.	11 35 33	with a 2 feet reflector of Short's. Air a little hazy.
July	1	Em.	9 50 24	with a 2 feet reflector of Short's. Jupiter very clear.

ECLIPSE OF THE MOON.

Observed with a 3½ feet achromatic treble object glass telescope of Dollond, with least magnify ng power 30 times; and a 2 feet reflector of Short's, with least magnifying power 60 times.

1769	Apparent time. h ′ ″	h ′ ″	
Dec. 12	16 57 13	16 57 9	Beginning of eclipse.
	17 3 3		The shadow first touches Kepler.
	17 3 33		bissects ditto.
	17 4 3	17 3 35	covers ditto.

Apparent

	Apparent time.		
1769	h ′ ″	h ′ ″	
Dec. 12	17 9 19		The shadow touches Pytheas.
	17 10 32	17 10 30	covers ditto.
	17 13 22		covers Timochares.
	17 15 54	17 15 18	covers Archimedes.
	17 16 56		touches Eratosthenes.
	17 18 20		covers ditto.
	17 20 1		bissects Copernicus.
	17 26 1	17 26 5	touches Mare Serenitatis.
	17 30 52		touches Manilius.
	17 32 58	17 33 20	covers ditto.
	17 35 17		touches Menelaus.
	17 36 17	17 37 5	covers ditto.
	17 39 42		touches Plinius.
	17 40 17	17 40 43	covers ditto.
		17 45 14	covers Dionysius.
	17 53 9	17 53 38	covers a spot between Plinius and Promont. Somnii.
	18 6 52	18 6 55	bissects a black spot surrounded by a white circle in Terrâ Mannæ, lying in a leni joining Langrenus and Kepler.

The observations in the first column were made by myself, with the $3\frac{1}{2}$ f. achromatic telescope; and the others by my assistant W. Bayley, with the 2 f. reflector.

OCCULTATIONS OF FIXED STARS BY THE MOON.

	Apparent time.	
	h ′ ″	
April 11	7 24 11	Em. of μ Geminorum, from the Moon's bright limb. Certain to 5 or 10 seconds, with Dollond's $3\frac{1}{2}$ feet telescope.
May 9	8 14 49	Em. of ζ Geminorum, from the Moon's bright limb, with a 2 feet reflector. The true emersion is supposed to have been about 5 seconds sooner.

Mean

		Mean time. h ′ ″	
Sept. 15		8 1 43,3	im. of 16 Piscium, at ☽'s bright limb, with 6 f. reflector, certain to 1″.
Sept. 20	between	10 22 47,3	and 49″,3 with 6 feet refl. } im. 2d ϰ Tauri at ☽'s bright l.
	between	10 22 50,3	and 51″,3 with 2 f. ref. by W. B. } im. 2d ϰ Tauri at ☽'s bright l.
		10 28 19,6	im. of 1st ϰ Tauri, with both telescopes exactly the same.
		11 12 28,2	em. of 1st ϰ Tauri, at ☽'s dark limb, with 2 feet reflector.
		11 16 54,5	em. of 2d ϰ Tauri, with both telescopes exactly the same.
Sept. 25		17 24 14,6	im. *b* Leonis, at ☽'s bright limb, with 6 feet refl.
		17 24 12,6	ditto, with 2 f. reflector, by W. B.

The star was eclipsed to the north of the Moon's center, and the time seemed certain to less than a second, with both telescopes.

Nov. 18	14 39 3,7	im. of 2d α Cancri, at ☽'s bright limb, with 2 f. refl. Uncertain to 10″.
	15 5 49,3	em. of ditto, at ☽'s dark limb, instantaneous, and the same with both telescopes.

Read December 1, 1769.

LVI. *Eclipses of* Jupiter's *First Satellite, with an Eighteen Inch Reflector of Mr.* Short's. *Observed by Dr.* Wilson *at the* Glasgow *Observatory.*

				Equal time.				Apparent.				
				h	′	″	‴	h	′	″	‴	
1762	Sept.	11	Im.	10	55	33	21	10	55	33	21	good.
	Oct.	4	Im.	11	1	58	—	11	13	22	21	good.
	Nov.	14	Em.	6	8	33		6	23	43	55	indifferent.
		21	Em.	8	1	31	38	8	16	39		good [ly clear.
1763	Oct.	23	Im.	12	50	24	—	13	5	56	53	good, air extreme-
	Nov.	1	Im.	9	12	18	43	9	28	29	18	good, like the for-
1764	Oct.	25	Im.	16	8	48	—	16	24	40		indif. good. [mer.
1765	Febr.	14	Em.	6	18	28		6	3	55		good.
	March	7	Em.	12	2	24		11	51	29		good.
	Nov.	29	Im.					15	53	26		indifferent.
	Dec.	22	Im.	15	54	4	—	15	54	25		indifferent.
1766	April	20		8	4	23	—	8	5	41	15	good.

	Solar Eclipse.	h	′	″	h	′	″	
1769 March 31	Beginning	20	58	16	20	54	15	9
	Gr. obs.	22	19	3	22	15	2	
	End	23	46	55	23	42	52	

1769 Nov. 6

☽'s west. limb. Merid.	Alt. of ☽'s under limb.	Parts of 96.
h ′ ″	° ′ ″	° ″ parts.
6 52 6	20 47 36+	22 1 14

Barom.	Ther.	without.	
inches	°	°	
29,66	49	46	This is the only one of the Moon since the quadrant was in order.

N. B. The equation of time made use of, is taken from De la Caille's Ephemeris.

Obser-

Observations of Eclipses of Jupiter's *First Satellite, proper to be compared with the foregoing ones, in order to determine the Difference of Meridians of* Greenwich *and* Glasgow. *Communicated by the Astronomer Royal.*

				App. time. h ′ ″	
1762	Sept.	11	Im.	11 12 43	Surry-street, 2 feet reflector. By N. Maskelyne.
	Oct.	4	Im.	11 31 1	Surry street, 2 f. ref. Observed by J. Short.
	Nov.	3	Em.	15 56 57½	Greenwich, 6 f. ref. By C. Green.
	Nov.	12	Em.	12 10 56	Surry-str. 2 f. ref. By J. Short.
1763	Oct.	16	Im.	11 27 39	Surry-str. 2 f. ref. By J. Short.
	Nov.	1	Im.	9 45 25	Surry-str. 2 f. ref. By J. Short.
1764	Nov.	4	Im.	13 3 37	Greenwich, 2 f. ref. By C. Green.
	Nov.	10	Im.	14 57 11	Greenwich, 6 f. ref. By C. Green.
1765	Feb.	19	Em.	13 46 42	Greenwich, 6 f. ref. By C. Green, good observation.
	Dec.	1	Im.	10 40 11	Greenw. 18 inch ref. By N. Maskelyne
		8	Im.	12 31 34	Greenw. 6 f. ref. Air very clear. By N. Maskelyne
		15	Im.	14 22 3	Greenw. 6 f. ref. By N. Maskelyne
		22	Im.	16 12 19	Greenw. 6 f. ref. By N. Maskelyne
		24	Im.	10 39 27	Greenw. 6 f. ref. By N. Maskelyne
1766	April	11	Em.	11 56 30	Greenw. 6 f. ref. By J. Dymond.

N. B. The late Mr. Short's house, in Surry-street, where some of the above observations were made, is 26″½ of time west of Greenwich.

LVII. *Extract of a Letter to the Reverend* Nevil Maſkelyne, *Aſtronomer Royal, from Mr.* Benedict Ferner, *F. R. S. Dated* Stockholm, June 9, 1769. *Tranſlated from the* French.

S I R,

Read Dec. 21, 1769.

I AM more ſurprized that the times of the contacts of Venus and the Sun's limbs, obſerved here, by different obſervers, with different inſtruments, agree ſo near together, than I am at their difference; for the nearneſs to the horizon, and the extraordinary quantity of vapours with which the atmoſphere was then loaded, not only cauſed the limb of the Sun to tremble and undulate, but alſo gave it, if I may ſo expreſs myſelf, the form of a large ſaw, the eminences being luminous and the cavities black, which ſhifted places like the waves of a tempeſtuous ſea.

There was no reaſon for fixing the moment of the ingreſs of Venus ſooner than ſhe had made a greater cavity in the limb of the Sun than the depth of the waves or black notches; and then one might be very ſure of the fact: but certainly at that time ſome ſeconds muſt have been paſſed from the beginning of the ingreſs. Therefore, I am

am very well perſuaded, that $8^h\ 24'\ 9''$ apparent time, which I took for the beginning of the ingreſs, is four, five, or ſeven ſeconds too late. I hoped to ſee the internal contact with more certainty; but I was miſtaken; for I found as great difficulties there, though of another kind. When I judged, by means of the circular figure of the Sun's diſc, that Venus ſhould be intirely within the Sun, I could not yet ſee the luminous cuſps of the Sun join together behind Venus, who, on the contrary, appeared to carry the limb of the Sun along with her, which appeared to bend towards Venus, leaving a black cavity in his limb; and a moment after, when I thought I ſaw the whole body of Venus in the Sun, a little black column appeared to proceed from Venus towards the imaginary limb of the Sun. The whole of this phænomenon was certainly, in my opinion, the effect of the tremors of the limbs of the Sun and Venus; but I took $8^h\ 41'\ 48''$ for the moment of the internal contact, when the thread of the Sun's light cloſed behind Venus.

The limbs of Venus were, at leaſt, as tremulous and ill defined as thoſe of the Sun. Sometimes Venus had black eminences, which projected ſo much that they were not unlike a pointed truffle. The firſt notch made by Venus in the Sun was not round, but reſembled an obtuſe angle. The diameter of Venus, which was perpendicular to the Sun's limb, appeared the greateſt while Venus was paſſing over the Sun's limb; but after Venus had paſſed the Sun's limb, the ſame diameter appeared the ſmalleſt; ſo that Venus preſented herſelf in both theſe caſes under an oval form, but in contrary directions.

Clouds

Clouds hindered us from obſerving the beginning of the eclipſe of the Sun; but I obſerved the end of the eclipſe, at $10^h\ 4'\ 53''$ apparent time, with an achromatic teleſcope of Dollond, of 10 feet, magnifying 96 times; the ſame teleſcope which I uſed in obſerving the tranſit of Venus. The difference of meridians between Stockholm and Upſal is $1'\ 40''$ of time.

I have the honour to be,

with the greateſt friendſhip and eſteem,

SIR,

Your moſt obedient,

humble ſervant,

B. Ferner.

LVIII. *Obſer-*

LVIII. *Obſervations of the Tranſit of* Venus *over the* Sun, *on* June 3, 1769; *and the Eclipſe of the* Sun *the next Morning; made at* Eaſt Dereham, *in* Norfolk, *by the Rev.* Francis Wollaſton, *F. R. S.* *Extracted from ſome Letters addreſſed to the Rev.* Nevil Maſkelyne, *F. R. S.* *and Aſtronomer Royal.*

Read Dec. 21, 1769.

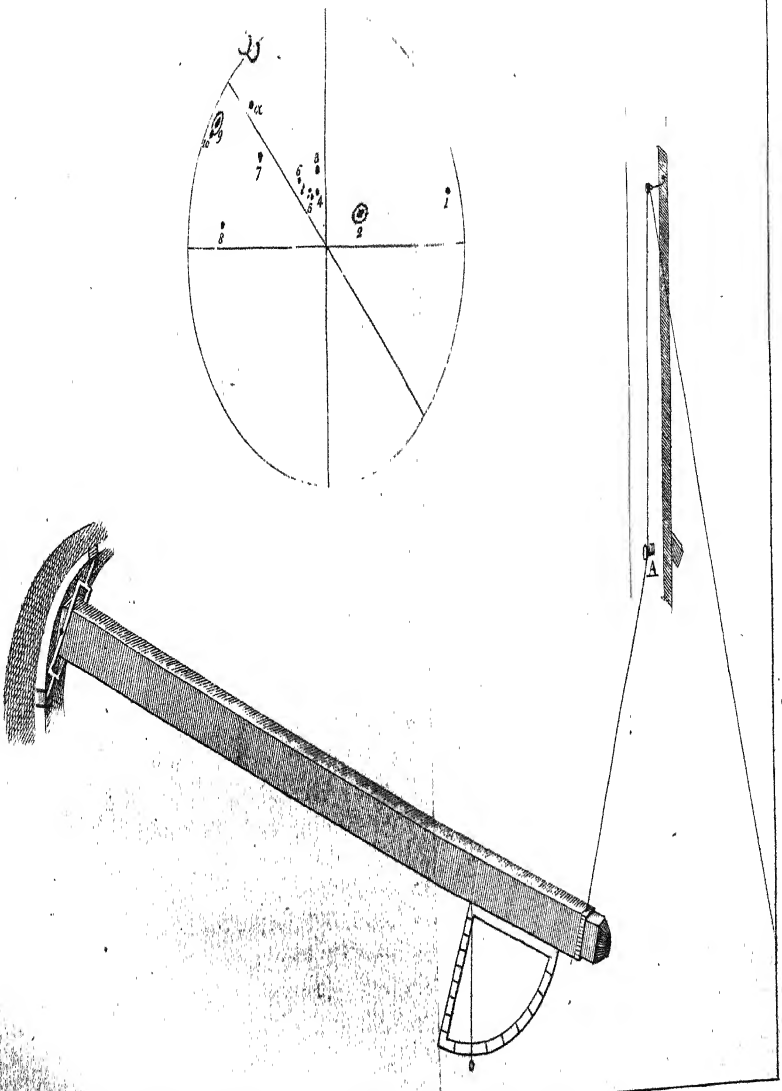
THE teleſcope I uſed was a reflector of Short's, of 12 inches focus, with a power that magnifies about 55 times. My clock was made by Holmes. It eſcapes dead ſeconds, has a pendulum with a wooden rod, and is firmly faſtened to a ſtack of chimnies in the room where I obſerve. I had lately received it from London, and regulated it by tranſits of the Sun and ſtars, taken with a tranſit inſtrument of a peculiar kind (deſcribed at the end of this account); and alſo by equal altitudes of the Sun, taken with another inſtrument of my own make. By ſeveral tranſits of the Sun and fixed ſtars, obſerved in the latter end of May and the beginning of June, I found I had brought it to go at the rate of mean ſolar time very nearly, *i. e.* loſing but 1″ in 14 days; and by tranſits of the Sun, it appeared to be 1″,5 too ſlow for mean time, on the third of June. But by a mean

of

of 8 days obſervations of equal altitudes, taken in June and the beginning of July, the clock appeared $5''$,2 ſlower than was found by the tranſit-teleſcope, which was not perfectly adjuſted; though, as its error continued always the ſame, it was very ſufficient for determining the rate of the going of the clock. Hence, on the day of the tranſit, the clock was $6''$,7 or $7''$ too ſlow for mean time; and the obſervations that follow are corrected accordingly.

In watching for the firſt contact of Venus, I kept my eye on the Sun's edge where the contact was expected; keeping that point nearly in the center of my field; and the firſt impreſſion which I ſaw (without any penumbra or atmoſphere that I could perceive) was at $7^h\ 12'\ 32''$ by my clock, or $7^h\ 12'\ 39''$ mean ſolar time. I looked particularly to ſee whether the reſt of the Planet were viſible while only part was on. If I ſaw that, it appeared as a portion of a larger circle: but I think I ſhould not have obſerved it at all, if I had not looked for it. The dark part on the Sun did not appear to me with a ſmooth edge; and yet I could not diſcern any undulation in it: but the clouds obliged me to ſlide my ſmoaked glaſſes ſo often, it interrupted me much. I then tried my ſtrongeſt magnifying power of 110, but to no purpoſe; for there was an undulation on the Sun's edge, by that time, ſo great, that I thought it beſt to return to the former power of 55. Before the internal contact, at about $7^h\ 24'$, I loſt the Sun entirely, and, though there were a few breaks in the clouds, he never appeared more that evening.

As to the eclipſe the next morning, I ſaw that more perfectly; though at firſt there were many flying


α
a
1
2
4
5
6
7
8
9
10
A

flying clouds; which, however, did not deprive me of any obſervation I was capable of making. It began here at $18^h\ 42'\ 17''$ by the clock, or $18^h\ 42'\ 24''$ mean time (a cloud had juſt paſſed; but I can be ſure it was not viſible $2''$ or $3''$ before); and it ended at $20^h\ 28'\ 1''$ by the clock, or $20^h\ 28'\ 8''$ mean time. I uſed the magnifying power 55, as I had done for the tranſit. As I have no micrometer, the beſt method I thought I could take to obſerve the phaſes of the eclipſe, was by the immerſions and emerſions of the ſpots; which, if there were comparative obſervations in other places, might meaſure them better than any inſtruments I have. The ſcheme annexed ſhews the ſituation of the ſpots, as I judged of them by my eye; and the obſervations that follow refer to the numbers in the drawing, TAB. XVII. fig. 1.

Mean time.

h	′	″	
18	42	24	Beginning of the eclipſe.
	44	37	Spot 1 immerges.
19	3	17	The nebula of ſpot 2 begins to immerge.
	4	47	Spot 2 quite immerged, but not the nebula.
	14	56	Spot 3 immerges.
	16	44	Spot 4 immerges.
	18	50	Spot 5 immerges,
	19	29	Spot 6 immerges.
	24	56	Spot 7 begins to immerge.
	25	22	Spot 7 immerges entirely.
	30	21::	Spot 1 immerges.

Mean time.

h	′	″	
19	33	50	Nebula of ſpot 9 begins to immerge.
	34	14	Spot 9 begins to immerge.
	34	51	Spot 9 immerges entirely.
	35	3	Spot 10 immerges.
	42	31	Spot 8 immerges.
	45	8	Nebula of ſpot 2 begins to emerge.
	45	52	Spot 2 emerges entirely.
	46	57	Nebula of ſpot 2 emerges entirely.
	59	22	Spot 4 emerges.
20	0	10	Spot 5 emerges.
	1	38	Spot 3 emerges.
	4	50	Spot 6 emerges.
	11	35	Spot 8 emerges.
	24	53	Spot 9 emerges.
	26	22	Spot 10 emerges.
	28	8	The eclipſe ends.

The ſpots α and N° 8 were not viſible (or overlooked) on the Saturday morning, when I took a drawing of the others; but I think I may be ſure were both ſeen that evening. The ſpot α certainly was not viſible the next morning, though N° 8 and all the reſt were: and, as far as I can recollect, that was its true ſituation. But, as the Sun did not break out till a few minutes before 7, and was quite gone before the internal contact, I had no opportunity to watch it. The roundneſs and blackneſs of that ſpot, at firſt ſuggeſted a ſuſpicion of its being a ſatellite of Venus: but as this is not corroborated by any other accounts, it is probably a miſtake. The two wires at right

right angles to each other, exprefs a horary circle, and a parallel of the equator (the telefcope moving in the plane of the equator); and ferve to fhew the fituation of the fpots in general. The other is an oblique wire, the correfponding one having been taken out a few days before, in order to leave the field more clear.

As to my latitude, I have tried it different ways with fuch inftruments as I have; but what I trufted to moft, were the zenith diftances I took of α Cephei and α Cygni at one time; and η and ζ Urfæ Majoris at another (for fome nights fucceffively) with my tranfit telefcope; from which I fufpend a plummet, at a certain diftance from the quadrant underneath, one of whofe radii becomes thereby a tangent; which being graduated, I calculate by it the diftance of ftars near the zenith, and correct the errors of collimation by taking them both ways. I have likewife taken ftars both ways, with my equal altitude quadrant, which indeed is a very imperfect inftrument for that purpofe: but, by calculating the mean, it turns out much the fame as by the other method, viz. 52° 40′ 20″ north.

The other figure (2) is a fketch of my tranfit-telefcope, which you enquired after. The principle it goes upon is this: the center of the object-glafs remains immoveable, while the telefcope (by crofs-axes at right angles to each other, which interfect at that center) hangs as it were from thence, and has a free motion every way; and particularly its motion eaft and weft is freed, as much as poffible, from friction. At the focus, where the crofs wires are,

 it

it is ſuſpended by a ſmooth even catgut, by which that end is let down to the altitude required. This catgut bears conſtantly againſt a deep angular groove that is cut round the end of the ſcrew marked A; by which means that end hangs always from the ſame point at all altitudes; from whence, the centre of gravity of the teleſcope keeps it conſtantly in the ſame plane. This ſcrew runs eaſt and weſt; and therefore, when the teleſcope is drawn up to a horizontal poſition, ſerves to adjuſt it to the meridian, by directing the wires to a diſtant mark. The ſettling of that meridian mark is certainly the ſame in this as in other tranſit-inſtruments; and perhaps mine is not exactly right. But I ſuſpect my inſtrument errs in another particular; that the line of collimation does not run parallel to the interſection of croſs axes and the center of gravity; which it ought to do. This is owing to bad workmanſhip, which a ſkilful operator could eaſily ſet right; and, when adjuſted, I ſhould think would not be apt to vary, but would always continue to deſcribe a true vertical, as indeed it has proved upon trial; having always, in this rude form, kept to the ſame errors at the ſame altitudes.

The reſt of the drawing, I believe, explains itſelf: excepting the two curved pieces of paſteboard between which the teleſcope is directed; which are deſigned to keep off the air that would make it vibrate; and do in effect take off alſo a great deal of falſe light in the day time, and anſwer the purpoſe of an illuminator at night.

The

The thought occurred to me while I was uſing Lord Charles Cavendiſh's meridian line; to which you ſee I thus adapt a teleſcope; and there being fewer particulars neceſſary to be attended to, when once rectified, I thought it would be more ſerviceable in a common building, than thoſe tranſit-inſtruments which require ſo very ſteady a foundation.

East-Dereham,
June and July, 1769.

Francis Wollaſton.

LXIX. *Obſervations of the Tranſit of* Venus *over the* Sun, June 3, 1769; *made by Mr.* Owen Biddle *and Mr.* Joel Bayley, *at* Leweſtown, *in* Pennſylvania. *Communicated by* Benjamin Franklin, *LL.D.F.R.S.*

Read Dec. 21, 1769.

ON the 26th of May, 1769, Joel Bayly and myſelf arrived at Leweſtown (on Cape Hinlopen at the mouth of Delaware Bay), being ordered there, by the American Philoſophical Society, held at Philadelphia, for promoting uſeful knowledge, to take an obſervation of the enſuing tranſit of Venus over the Sun's diſc; and immediately ſet about fixing our time-piece, in a houſe (which we hired) on the ſouth ſtreet of the town, where we were moſt likely to be free from interruption, and had an open view of the Sun and ſtars for our obſervations. We ſet a ſtrong oak poſt in the ground, to which we ſcrewed our clock caſe, reſting the baſe of it on the ground, the face of it fronting a door which opens to the ſouthward, ſo as to be convenient for us to hear the beat of the clock, where we intended to fix our teleſcopes and inſtrument for taking equal altitudes. We then ſet a poſt in the ground for the equal altitude inſtrument, which was not

not ſo good as I wiſhed, but much better than a Hadley's quadrant, as we found by experience, and the beſt we could procure in time for our purpoſe. It was a theodolite, with teleſcopic ſights, in which there were croſs hairs; it had a ſpirit level to adjuſt the plain of the inſtrument horizontally; and alſo one applied to the teleſcope parallel with its axis, and at right angles to the other ſpirit level. By means of theſe two levels and adjuſting ſcrews, we found we could adjuſt it very nearly, the inſtrument being a very good one of the kind. With this we ſet our clock, by taking equal altitudes of the Sun, which we corrected by the tables publiſhed in a pamphlet, intitled, Inſtructions relative to the enſuing Tranſit of Venus, &c. by the Rev. Mr. Nevil Maſkelyne. The 27th, we got ſome good correſponding altitudes of the Sun, by which we ſet our clock; and took equal altitudes of ſome of the fixed ſtars, to prove the rate of our clock. After this it continued cloudy, with rain at times, and a high wind at north-eaſt, till the 31ſt, when the clouds broke a little. During this time, we employed ourſelves in meaſuring the diſtance of our place of obſervation from the ſtone fixed at the beginning, or eaſt end, of the eaſt and weſt line, which is the boundary between the three lower counties and Maryland, and is ſituate on Fenwick's Iſland; the latitude and longitude of this place being accurately determined by Meſſieurs Dixon and Maſon.

The meridional difference of the latitude of the place of our obſervation, north from Fenwick's Iſland, at the beginning of the eaſt and weſt line, as before deſcribed, being the eaſternmoſt end of the ſouthern

boundary

boundary between the lower counties and Maryland, is 19′ 41″ 24‴; and the meridional difference of longitude of the place of our obſervation, weſt from the point aforeſaid, in Fenwick's Iſland, 5′ 45″ of a degree. Theſe data, with the latitude and longitude of Station Point, will determine exactly the place of obſervation.

June 2, the weather being clear, had good correſponding obſervations of the Sun.

June 3, the weather being remarkably fine, had good obſervations to ſet our clock. About 12 o'clock began to direct our glaſſes to the Sun, keeping it continually in the field from then to the time the obſervation was paſt. We agreed to watch our teleſcope one minute in turn, till about ſeven or eight minutes before the contact was expected, leſt, by too ſteady an attention to the glaſſes, our ſight ſhould be impaired, ſo as to diſable us from diſcerning the contact clearly. I had left my teleſcope the minute preceding the contact, intending to apply myſelf ſteadily to it, from the next minute, until the obſervation was paſt; and when the 48th ſecond was called, I applied myſelf to the glaſs, and by the time three ſeconds were elapſed, I perceived, on that part of the Sun's limb where I expected the contact, a ſmall impreſſion, which proved to be the limb of Venus in contact with the Sun. All the limb of the Sun, which appeared at that time in the field of the teleſcope, had a ſmall undulatory motion, which, I apprehend, was owing to denſe vapours, which aroſe at that place, being near the ſea. At Venus's firſt appearance to me, it was only like one of thoſe waves on the limb or border of the Sun, increaſed in

ſo

ſo ſmall a proportion, that I remained doubtful for ſeveral ſeconds, whether it was any thing elſe; thus it continued, making a deeper impreſſion, with that tremulous motion, for about ten ſeconds, when the tremor where Venus was in contact ceaſed, and the indenture was truly circular, with an even termination.

My abſence from the teleſcope, juſt before the contact occurred, deprived me of an opportunity of judging whether there was any appearance of an atmoſphere preceding the weſtern limb of Venus in contact; but when Venus had entered nearly one half of its diameter into the Sun's diſc, my companion and myſelf ſaw a luminous creſcent, which enlightened that part of Venus's circumference which was off the Sun, ſo that the whole of her circumference was viſible, but did not continue ſo until the internal contact; and at the time of the firſt internal contact, the eaſtern or external limb of Venus ſeemed to be united to the Sun's limb by a black protuberance or ligament, which was not broke by the entrance of the thread of light, till four ſeconds after that the regular circumference of Venus ſeemed to coincide with the Sun's.

The teleſcope I made uſe of for viewing the tranſit, was a reflecting one, belonging to the Philadelphia Library Company, the ſpeculums of which are 2½ feet apart, and the lenſes in the eye tube four inches apart; it was the leaſt magnifying power that I uſed, as I found the tremulous motion too much magnified by the other power. The ſmall one was in good order, and defined the Sun's limb, and ſpots on its diſc, very clearly. I had applied a polar axis to

to it, and made ſome rack-work, by which I could keep the ſame part of the Sun's limb in the field with eaſe; my companion was not ſo well provided with a teleſcope, the one he uſed being of Dollond's refracting glaſſes of 4½ feet. This we fixed, with a ball and ſocket, to a poſt, by which it was eaſily directed to the Sun. Thus furniſhed, we found the contacts to take place as follows, reduced to mean time.

		h	′	″
Owen Biddle's	External contact at	2	11	53
	Internal one at	2	29	53
Joel Bayley's	External contact was loſt by an accident, but ſeen by him, after it had taken place, at	2	12	15
	Internal ditto	2	29	53

It muſt be noted, the internal contact, given by Owen Biddle, is at four ſeconds before the thread of light had broken the dark ligament or protuberance, by which Venus's limb was united to the limb of the Sun, that being the time he eſtimated the two limbs to be in contact.

The internal contacts, we think, may be relied on; the external happening ſooner than expected, occaſioned a doubt at its appearance, which made the exact ſecond of its appearance a little uncertain.

Signed,

Philadelphia,
June 9, 1769.

Owen Biddle.

Copy taken in haſte, but the times examined by Owen Biddle.

The

The times of the contacts of Venus with the limb of the Sun, as seen by Owen Biddle and Joel Bayley on Cape Hinlopen; with the true difference of latitude and departure of the place of their observation, from the Middle Point between Fenwick's Island and Chesopeak Bay, are as follows, viz.

	h	′	″	
External contact at	2	11	53	mean time
Internal contact at	2	29	53	

The difference of latitude of the place of observation, north of Middle Point.	21,93	miles
The meridian distance of the place of observation, east of Middle Point.	30,6356	miles

The latitude and longitude of Middle Point were taken by Messieurs Dixon and Mason, and, as we suppose, communicated to the Royal Society, but we are not yet acquainted with it.

N. B. As we are not acquainted with the exact measure of a degree of latitude, agreeable to the above gentlemen's measurement, we have sent the difference of latitude and longitude in miles and decimal parts, as it may be reduced to greater certainty thereby.

Remarks by the ASTRONOMER ROYAL.

From the data given above, and the length of a degree of latitude, found by Meſſieurs Maſon and Dixon, in theſe parts $=$ 68,896 Engliſh miles, the difference of latitude of Leweſtown and the Middle Point above mentioned (which is the ſame with the point A; ſee Meſſieurs Maſon's and Dixon's meaſure of a degree, Philoſ. Tranſact. vol. LVIII. p. 276) is $19' 53''$; but the latitude of the point A was found, by Meſſieurs Maſon and Dixon $38° 27' 34''$; therefore that of Leweſtown is $38° 47' 27''$ north; and the difference of its meridian, and that of the point A, or their difference of longitude, is $34' 0'' = 2' 16''$ of time, Leweſtown being to the eaſt. But if the difference of longitude of Leweſtown eaſt of the Stones on Fenwick's Iſle be ſuppoſed truly given, in the former account, $5' 45''$ of a degree, then the difference of longitude of Leweſtown and the point A will come out about $1'$ of a degree, or $4''$ of time leſs; for Mr. Dixon acquaints me, that the diſtance of the Stone on Fenwick's Iſle, eaſt of the point A, is 35 Engliſh miles wanting 100 yards. Now this is equal to $30' 26''$ of a great circle $= 38' 51''$ of longitude; from which ſubtracting $5' 45''$, there remain $33' 6''$ for difference of longitude of Leweſtown and Point A $= 2' 12''\frac{1}{2}$ of time, or $3''\frac{1}{2}$ leſs than found before; and this latter I take to be neareſt the truth. If this be ſo, Leweſtown is very nearly under the ſame meridian with the ſouthernmoſt part of the city of Philadelphia, or more accurately $13''$ of longitude, anſwering

anſwering to 1″ of time, eaſt of it. For, by Meſſieurs Maſon's and Dixon's meaſure of a degree, the point N (ſee Philoſ. Tranſact. Vol. LVIII. p. 276) is 2′ 19″ of longitude weſt of the point A; and N, by meaſurement, is 31 Engliſh miles due weſt of the ſouthernmoſt part of the city of Philadelphia, anſwering to 35′ 12″ of longitude; from which ſubtracting 2′ 19″, there remain 32′ 53″, anſwering to 2′ 11″½ of time, for the difference of longitude of the ſouthernmoſt part of Philadelphia, eaſt of the point A. But Leweſtown is found above to be 33′ 6″ of longitude = 2′ 12″½ eaſt of the point A, and conſequently is 13″ of longitude, or about 1″ of time eaſt of the ſouthernmoſt part of the city of Philadelphia.

Nevil Maſkelyne.

LX. *Obſer-*

LX. *Obſervations of the Tranſit of* Venus *over the* Sun, *made at the Round Tower in* Windſor Caſtle, June 3, 1769. *By* Daniel Harris, *Maſter of the Royal Mathematical School in* Chriſt's Hoſpital, *and F. R. S. In a Letter to the Reverend* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal.*

SIR,

Read Dec. 21, 1769.

I HAVE taken the liberty to ſend you my obſervations on the tranſit of Venus, as likewiſe thoſe made for aſcertaining the going of the clock; all which I ſhould have done laſt June, when I ſent the times of the contacts, to the end that they might, if you thought them worthy, have been communicated to the Royal Society through your hands; but, waiting for an opportunity of uſing your tranſit-inſtrument, in order to aſcertain the longitude of Windſor, which you was ſo obliging to lend me for that purpoſe, prevented; and am ſorry ſtill to ſay, that I have not once been able to make uſe of it, on account of the badneſs of the weather at the ſeveral times I took it down with me for that purpoſe. However, other methods have been

been made uſe of, and no pains ſpared for aſcertaining that material point, in which I hope I have ſucceeded, as well as in making the other neceſſary obſervations; and ſhall be happy if they meet with your approbation, and in any way tend to aſſiſt in determining the grand point in queſtion.

My ſituation for obſerving the tranſit, and making the previous obſervations, was extremely advantageous; all of which were made within hearing of the clock, which was a good regulator, fixed up in the Round Tower a fortnight before, by permiſſion of the Governor, his Grace the Duke of Montagu; who was ſo obliging, upon my worthy friend Captain Alexander Schomberg's application to him, by letter, in both our names, immediately to give his conſent, with ſtrict orders to his ſervants to take care that we were not diſturbed in making our obſervations, particularly on the day of the tranſit; which orders were moſt punctually obeyed; nobody being admitted into the Round Tower on that day but ourſelves, and two others, as aſſiſtants to watch the clock; viz. the Reverend Doctor Boſtock, Canon of Windſor, and the Reverend Mr. James Townley, Head-maſter of Merchant Taylors School; both gentlemen acquainted with the nature and uſe of aſtronomical obſervations.

The regulator was fixed truely perpendicular, and well faſtened to the wall and floor of the room where the tranſit was to be obſerved, and obſervations made on its rate of going for ſeveral preceding days, both by Captain Schomberg, an officer, well acquainted with aſtronomical obſervations, and myſelf; by which we found that it loſt at the rate of

twenty

twenty ſeconds per day nearly. The greateſt part of the altitudes taken for that purpoſe, with a good Hadley's quadrant, in a ſaucer of treacle and water, covered with your glaſs roof, when neceſſary, to ſcreen it from the wind, which I found to be of great uſe, are herewith incloſed, with their correſponding times, &c. which have all been compared ſeparately, in compliance with your requeſt, both by Captain Schomberg and myſelf; and which I hope will be found to be ſatisfactory.

As to the longitude of Windſor Caſtle from Greenwich, which has ſo long engaged my attention, though I have not had an opportunity of aſcertaining it with your tranſit-inſtrument, by the method of differences of azimuths, which I have long wanted to do, yet, notwithſtanding, by a mean of ſeveral bearings of St. Paul's, taken from the corner of the terras, near the dial, with a good theodolite, and found to be N. 82° 30′ from the true meridian, and the difference of latitude between that cathedral and Windſor Caſtle $2\frac{1}{2}$ geographical miles, think it may be very nearly determined; and in the following manner.

The latitude of St. Paul's, or, which is the ſame thing, of the Royal Mathematical School in Chriſt's Hoſpital, by the mean of a great number of obſervations, I make to be 51° 30′$\frac{3}{4}$ N. and by a mean of ſeveral double altitudes of the Sun, taken in a ſaucer of treacle and water, ſcreened from the wind, I find the latitude of Windſor Caſtle to be 51° 28′$\frac{1}{4}$ N. the difference of latitude therefore between thoſe two places is $2\frac{1}{2}$ geographical miles; with which, and the bearing of St. Paul's from the Caſtle N. 82° 30′ E.

(variation

variation 20½ degrees allowed for), I make the difference of longitude between them (by Mercator) to be 30½ miles, which is equal to 2′ 2″ of time: and recollecting your mentioning to me Dury and Bell's actual Survey of London and its environs for 30 miles, as of ſome uſe for the purpoſe, I have looked over it, and find by that, that the direct diſtance between Windſor Caſtle and St. Paul's, London, is 22 ſtatute or meaſured miles: and by another Survey of the ſame kind, done by Kitchen, I find the diſtance between thoſe two places to be very nearly the ſame. Therefore, with this diſtance of 22 miles, equal to 19 geographical ones, and the difference of latitude, by obſervation, between the two places 2½ miles, I find the departure to be 18,8 miles, which gives 30,2 miles of longitude, equal to 2′ 1″ of time, agreeing within a ſecond to the former method.

The difference of longitude, or difference of meridians, therefore, between the Round Tower Windſor Caſtle, and St. Paul's, London, I think I may venture to put at 30½ miles, or 2′ 2″ of time; though I am perſuaded, if any thing, it is rather more than leſs; to which if we add the difference of longitude in time between St. Paul's and Greenwich, which is 22″½, it will give 2′ 24″½ of time for the difference of longitude between the Round Tower at Windſor Caſtle, and the Royal Obſervatory at Greenwich.

I cannot help obſerving, that the only inconvenient circumſtance, during the time of obſerving the tranſit, was the wind; which, blowing rather hard, and directly into the teleſcope, together with the ſmallneſs

of the Sun's altitude at that time, made the limb so very ill defined and undulating, that it is possible there may be an error of five or six seconds, at least, in the time of the external contact: being anxious, therefore, of having the internal contact as exact as possible, I changed the magnifying power of my telescope from that of 125 times, recommended by yourself, to that of 55 times, the least of all, which succeeded beyond expectation; for by this means that undulating motion of the Sun's limb was greatly reduced, though not entirely taken away, appearing much better defined than before, as did likewise that of the Planet Venus; insomuch that the error, if any, in the time of the internal contact, by which I mean the completion of the thread of light formed by the Sun's circumference, cannot exceed three seconds. The observing of the two contacts with so different magnifying powers as that of 125 times and that of 55 times, must occasion some difference in the times, and duration between the two contacts, to what they would have been, had they both been observed with the same magnifying power; which is to be allowed for.

Venus appeared remarkably protuberant on her upper limb, both before and at the time of her internal contact, which went gradually off soon after, but did not, though I earnestly attended to it, observe any thing like an atmosphere about her.

Be pleased, Sir, to accept of my best thanks for the use of your glass roof and transit-instrument, as likewise for your very obliging and useful communications, at different times, on the present subject,

ſubject, which I ſhall always have the moſt grateful ſenſe of; and am, with real eſteem,

SIR, Your much obliged,

and moſt obedient, humble ſervant,

Royal Mathematical School,
in Chriſt's Hoſpital,
Dec. 21, 1769.

Daniel Harris.

Times of the contacts of Venus with the Sun, as obſerved from the Round Tower, in Windſor Caſtle, by permiſſion of his Grace the Duke of Montagu, June 3, 1769.

Latitude 51° 28′¼ N. and longitude 2′ 24″½ in time, W. from the Royal Obſervatory at Greenwich.

	By the clock. h ′ ″	Mean time. h ′ ″
The external contact of Venus with the Sun,	7 4 30	7 06 14 p. m.
The internal contact at	7 22 38	7 24 22
Duration between the contacts, the clock being 1′ 44″ too ſlow for mean time,		0 18 08
* Venus's diameter meaſured 3 different times		0 59½

Chords meaſured parallel to the equator.

Venus's weſtern limb from the Sun's eaſtern limb, at 7^h 46′ 04″ mean time	3 42½
Venus's eaſtern limb from the Sun's weſtern limb, at 7^h 47′ 04″ mean time	15 16
Neareſt diſtance of Venus's lower limb from the Sun's limb, at 8^h 0′ 15″, the laſt of her	2 49
The Sun's horizontal diameter (at 7^h 30′)	31 42

Note, Theſe obſervations were made with a good regulator, made by Binning, of Windſor; an 18 inch reflector, made by the late ingenious Mr. Short, and a double object glaſs micrometer, made by Dollond.

* The very ſame that I made it at the tranſit, 1761.

An account of the methods uſed to aſcertain the going of a clock, fixed up in the Round Tower at Windſor Caſtle, in latitude 51° 28′¼ N. from the 30th of May to the 3d of June following, 1769.

		′ ″
♂ May 30 h at 4¾ p. m.	By a mean of three double altitudes of the Sun's center, the limb not being well defined, taken with a good Hadley's quadrant, made by Adams, in a ſaucer of treacle and water, ſo placed within the room as not to be diſturbed by the wind, after having worked each ſeparately, I found the clock to be 3′ 18″ too ſlow for the Sun, and 0′ 25″ too ſlow for mean time.	
☿ May 31 at 8¾ a. m.	By a mean of four double altitudes, of the Sun's upper limb, after being worked ſeparately, found the clock too ſlow for mean time	0 40
	And by a mean of 3 correſponding altitudes the ſame afternoon	0 48
♃ June 1 at 5½ p. m.	* By a mean of 4 altitudes, worked ſeparately, too ſlow	1 00
♀ June 2 at 8¾ a. m.	By 9 different altitudes of the Sun's upper limb, all worked ſeparately, and taking the mean, found the clock too ſlow	1 16¼
	And, by a mean of 3 correſponding altitudes, the ſame day	1 17
♄ June 3 at 8¼ a. m.	By the mean of two altitudes only; the weather not permitting more, made the clock too ſlow for mean time	1 26
Ditto. at 4¼ p. m.	The ſame afternoon, the weather being extremely fine, by 4 more double altitudes of the Sun's upper limb, worked ſeparately, and a mean taken, found the clock too ſlow for mean time	1 42

* Wound up the clock juſt before theſe altitudes were taken, which might affect it ſomething, although a regulator.

By

♄ June 3 at $5^{h}\frac{1}{4}$ p. m.	By 4 more double altitudes an hour after, the weather being exceeding fine, tho' windy, which obliged me to use the glass roof, by working all of them singly, and taking a mean, made the clock too slow for mean time	′ ″ 1 43

By all which it appears, that the clock lost of mean time, from the 30th of May to the 3d of June, inclusive, at the rate of $19\frac{1}{2}$ seconds per day; and by the last set of observations, at the time of the transit, 1 second per hour.

Some of the foregoing double altitudes, with their corresponding times as shewn by the clock, and the results, are here subjoined.

♀ June 2 The weather very fine.

Times by clock.	Double alt. ☉'s u. l.	Cl. too slow.	
h ′ ″	° ′	′ ″	
At 8 25 55 a. m.	81 $06\frac{1}{2}$	3 41	Clock too slow for the Sun.
— 28 45	81 $58\frac{1}{2}$	3 43	
— 33 47	83 $30\frac{1}{2}$	3 49	
— 40 13	85 $19\frac{1}{2}$	3 39	
— 43 35	86 $23\frac{1}{2}$	3 49	
— 46 37	87 16	3 47	
— 54 35	89 35	3 51	
— 56 30	Ditto center	3 44	
— 58 07	D° lower limb	3 51	
		9)– 414	
By the mean of all, clock too slow for the Sun		3 46	
Equation of time		–2 $29\frac{3}{4}$	
Clock too slow for mean time		1 $16\frac{1}{4}$	

Corre-

Correſponding altitudes, taken the ſame day.

	Times by cl.	Dou. alt.				
	h ′ ″	° ′		h ′ ″	′ ″	
At	8 5 18	74 50	up. limb	3 47 44	1 $05\frac{1}{2}$	cl. too ſlow for m. time
	8 8 32	75 $54\frac{1}{2}$		3 44 01	1 20	
	8 11 44	76 53		3 40 38	1 $25\frac{1}{2}$	
					3)3 51	

By a mean of the three, cl. too ſlow for m. time	1 17

♄ June 3 The weather very cloudy, and likely to rain.

			D. alt.			
	h ′ ″		° ′		′ ″	
At	8 15 47	a. m.	78 16	up. limb.	3 49	cl. too ſlow for the Sun
	8 18 17		79 02	ditto.	3 43	

Cl. too ſlow for the Sun, by the mean	3 46
Equation of time	2 20
Clock too ſlow for mean time.	1 26

Again,

At	4 11 38	p. m.	67 $31\frac{1}{2}$	up. limb.	4 02	clock too ſlow for the Sun.
	— 14 21		66 44		3 51	
	— 16 51		65 54		4 01	
	— 19 01		65 14		4 03	
					4)15 57	

By the mean, clock too ſlow for the Sun	3 $59\frac{1}{4}$
Equation of time	— 2 17
Clock too ſlow for mean time	1 42

Again,			Double alt.					
h	′	″	°	′		′	″	
At 5	11	35	48	56	up. limb.	4	01	Clock too flow for the Sun.
—	14	05	48	08½		4	01	
—	16	19	47	28		3	59	
—	18	27	46	48		3	57	
						4)15	58	

Clock too flow for the Sun	3	59½
Equation of time	—2	16½
Clock too flow for mean time	1	43

Received July 8, 1769.

LXI. *An Attempt to elucidate two* Samnite *Coins, never before fully explained. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

I.

Good Sir,

Read Dec. 7, 1769.

THE first of the coins I propose to consider here is a Samnite denarius (see Tab. XVI. n. 1.) of Papius Mutilus, published by Sig. Olivieri and M. Pellerin, with the word SAFINIM on the reverse, in Samnite-Etruscan characters; an interpretation of which has, as I apprehend, been ineffectually attempted by the Marquis Scipio Maffei, Sig. Olivieri, Sig. Avvocato Passeri, and M. Pellerin. The other has the initial letter of the name of a town upon the reverse, indicating the place where it was struck. For that Etruscan coins

coins are ſometimes adorned with the initial letters of the names of towns, is a point that will admit of no diſpute amongſt the learned. No one will refuſe his aſſent to this, who is apprized, that it has been allowed by Sig. (1) Olivieri and Sig. (2) Avvocato Paſſeri, the two greateſt proficients in Samnite, or Samnite-Etruſcan, literature that have hitherto appeared. What is now remarked has been alſo obſerved by Sig. Gori, in (3) the very valuable work here referred to.

Theſe pieces, however, are of a ſpecies different from that of the coins of Papius Mutilus, Tiberius Veturius, and Ni. Luponius, of which I have largely treated in (4) ſome of my former papers. The firſt ſpecies appertains to cities, the other to the Samnite, or Italian, commanders, whoſe names they bear, for the moſt part, on the reverſe. Hence it ſeems, at firſt ſight, extremely probable, that the word SAFINIM, on the reverſe of Papius Mutilus's medal, now before me, cannot be equivalent to SABINI, THE SABINES, or SAMNITES, THE SAMNITES, as ſome (5) of the moſt celebrated

(1) *Una lettera del Sig.* Olivier. *al Sig. Abate Barthelemy, &c.* p. 29. In Peſaro, 1757.

(2) Jo. Baptiſt. Paſſer. Piſaurenſ. *De Num. Etruſc. Pæſtanor.* in *Symb. Litterar.* Vol. II. p. 30, 31. Florentiæ, 1748.

(3) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 428. Tab. CXCVI. CXCVII. Florentiæ, 1737.

(4) *Philoſoph. Tranſact.* Vol. LII. Par. i. p. 28—39. Vol. LVIII. p. 253—261. & alib.

(5) Annib. degli Abati Olivier. *Diſſertaz. ſopr. alcun. Medagl. Sannit.* in *Sag. di Diſſertaz. Accademich. &c. di Corton.* Tom. IV. p. 142, 146. in Roma, 1741. Scip. Maff. ap. Annib. degl. Abat. Olivier. ubi ſup. Peller. *Second Supplem. aux Recueils des Medaill.* p. 1—14. A Paris, 1766.

Etrufcan antiquaries of the prefent age have not fcrupled to affert; but muft be taken, agreeably to the nature and genius of fuch coins, for the name of one of the Italian generals, who diftinguifhed himfelf in the Social war.

In fupport of the former opinion, it has been alledged, that the Etrufcan plural mafculine termination, at the time of the Social war, was IM, the very fame (6) with the Hebrew; and that there is a moft peculiar and neceffary connection between the name SAFINIM and the (7) fymbol on the reverfe, which accompanies it. But neither of thefe reafons, which have been offered, or at leaft fuppofed to be true, by two very great men (8), will be admitted by the learned. For that the Etrufcan plural mafculine termination was not the fame with the Hebrew, and confequently IM, in any of the cafes, but at leaft fimilar to the Latin, if not the very fame with it, in the later ages, of which that of the Social war was one, is clearly evinced by the Etrufcan coins, with the words (9) ICVFINI, ICVVINI, or EVGVBINI, (10) TIANVR, TEANVR, or TEANOR, upon them, to omit other inftances of the fame kind, that might, with equal facility, be produced. And that the fym-

(6) Scip. Maff. ubi fup. M. Peller. ubi fup. p. 11, 12, 13.
(7) Peller. ubi fup. p. 5, 11, 12, 13, 14.
(8) Scip. Maff. & Peller. ubi fup.
(9) Anton. Francifc. Gor. *Muf. Etrufc.* Vol. II. p. 422. Florentiæ, 1737.
(10) *Numifm. Antiq. &c.* Thom. Pembroch. et Mont. Gomeric. Com. P. 2. T. 88. n. 3. *Philofoph. Tranfact.* Vol. LII. Par. i. p. 37.

bol

bol on the reverſe of the medal I am now upon has no neceſſary or immediate connection with the word SAFINIM, attending it, is, I conceive, plain from hence, that this very ſymbol appears on the reverſes of two other Samnite coins, one of the Veturian, the other of the Luponian family, by (11) me heretofore explained, with the names of two Italian generals upon them. From whence, by parity of reaſon, and analogy, it will likewiſe farther follow, that the word on the piece now before me may be fairly preſumed to be the name of a Samnite, or Italian, commander, famous for his bravery, and laudable conduct, in the Social war. This, I ſay, ſeems extremely probable, not only from the nature and genius of the coin itſelf, but likewiſe from the ſimilarity and analogy it bears to other (12) coins, with the names of Italian captains moſt evidently upon them, attended by the ſame ſymbol that occurs on the medal which is the object of my attention here. But the truth of what is here advanced will, as I apprehend, even to demonſtration, appear, if we conſider, with proper attention, the legend on the reverſe of a Roman denarius (ſee TAB. XVIII. n. 2, 3.) of the Servilian family, in conjunction with that on the reverſe of a Samnite coin of Papius Mutilus, to whom appertains the piece I am now endeavouring to throw ſome light upon. The two firſt of theſe

(11) *Philoſoph. Tranſact.* Vol. LVIII. p. 253—265. & Vol. LII. Par. I. p. 28—39.

(12) *Sag. di Diſſertaz. Accademich. di Corton.* Tom. II. Diſſertaz. 2. & Tom. IV. Diſſertaz. 4. p. 133—149. *Philoſoph. Tranſact.* Vol. LII. Par. i. p. 28—39. & Vol. LI. P. ii. p. 853—865.

 medals

medals are ſo perfectly (13) ſimilar, that were not the characters with which they are adorned different, and the caps or helmets worn by two Caſtors viſible only on one of them, they might abſolutely be conſidered as duplicates of the ſame coin (ſee TAB. XVI. n. 2, 3.). Now the Roman denarius has preſerved the legend SERVEILIM on the reverſe, and the Samnite one I propoſe to elucidate here the inſcription SAFINIM likewiſe on the reverſe, in Samnite, or Samnite-Etruſcan, letters. As therefore SERVEILIM is apparently equivalent to SERVEILI M., SERVEILIVS MARCI, or, in the Roman ſtyle, SERVEILIVS MARCI FILIVS; the legend SAFINIM may be conſidered as equivalent to SAFINI M., or SAFINIVS MARCI, which, in the Samnite mode of expreſſion (14), anſwers to the Latin, or Roman, SAFINI M. F. that is SAFINIVS MARCI FILIVS. Wherefore it undoubtedly pointed at one of the Italian heroes, famous for his conduct and bravery in the war carried on, towards the decline of the ſeventh century of Rome, by the confederated Italian ſtates, againſt the Romans.

That my interpretation of the legend on the reverſe of the Roman denarius is agreeable to truth, we learn from the legend on the reverſe of a ſimilar denarius of the Servilian family (ſee TAB. XVIII, n. 4.), now in my ſmall collection, which is formed of the letters P. SERVILIM F., that is P. SERVILIVS MARCI FILIVS. Whence, in conjunction with the Samnite denarius

(13) *Sag. di Diſſertaz. Accademich. di Corton.* Tom. IV. Diſſert. 4. p. 133, 134. In Roma, 1743.
(14) *Philoſoph. Tranſact.* Vol. LII. P. i. p. 35.

(ſee

(see TAB. XVII. n. 2, 3.), we may infer, that the word denoting SON, amongst the Samnites, was absorbed in the genitive case of the father's name, after the manner of the Greeks, as I (15) formerly supposed; and that, in this particular, they were even sometimes (16) imitated by the Romans (see TAB. XVIII. n. 3.)

With regard to the Safinian family, I shall beg leave to observe, that it was a family of pretty considerable note. We are told by (17) Sig. Olivieri, that C. Safinius had a hand in the seditions of L. Apuleius, which so much disturbed the repose of the republic, about the middle of the seventh century of Rome. The names of several members of this family occur in some of the Latin, or Roman, inscriptions, published by (18) Gruter and (19) Muratori, to omit what has been said on the same subject by other writers. Sig. Olivieri (20) supposes the Safinian family might not improbably have been of a Samnite origin, and the interpretation of the word SAFINIM given here seems to add no small weight to such a supposition. Nay, we learn from (21) some of the above mentioned inscriptions, that

(15) *Philosoph. Transact.* ubi sup. & not. (21).
(16) *Sag. di Dissertaz. Accademich. di Corton.* ubi sup.
(17) *Sag. di Dissertaz. Accad. di Corton.* Tom. IV. p. 144.
(18) Jan. Gruter. *Corp. Inscript.* ex *Recens. et cum Not. Joannis Georgii Grævii.* CCCLXXXVI. 4. CMLIII. 5. MLXXV. 2. & MXCII. 7.
(19) Lud. Ant. Murator. *Nov. Thesaur. Veter. Inscript.* &c. CCCIX. 3. MDXCIX. 6. MDCCXLIX. 2. & DCCCXLIX. 2. Vid. etiam *Ind. Univers.* Class. XVII. Mediolani, 1742.
(20) *Sag. di Dissertaz. Accademich. di Corton.* Tom. IV. p. 146.
(21) Jan. Gruter. ubi sup. CMLIII. 5. Lud. Ant. Murator. MDCCXLIX. 2. & *Ind. Univers.* Class. XVII.

M. or

M. or MARCVS, was a prenomen uſed by certain members of this family, which may be conſidered as an additional proof of the truth of what has been here advanced. It is true, we meet with no commander of this name in the hiſtory of the Social war. But then it is as true, that we have very little relative to this war handed down to us by the Roman hiſtorians. As the war of the Allies was exceeding bloody, and continued near four years; we may reaſonably preſume many of the Italian generals and principal officers to have been cut off in the courſe of it, and to have been ſucceeded by others, ſeveral of whoſe names have not been mentioned by thoſe writers who have only juſt touched upon that war. Had the hiſtories of the Social war written by Cornelius Siſenna, L. Lucceius, and others, whoſe works, excepting a very few ſmall fragments, are now entirely loſt, eſcaped the ravages of time, we might perhaps have met there at leaſt with the name of this general, if not ſome account of his exploits in that war. But as the matter now ſtands, we muſt be ſolely obliged to the very valuable denarius I have been conſidering, which has preſerved the name of a family not hitherto obſerved on antient coins, for the preſervation of his memory, through ſuch a ſeries of ages as has elapſed between the commencement of the Social war and the preſent time.

I muſt not forget to remark, that the ſmall (22) point between the two laſt letters of the word SAFINIM, on the coin, ſeems to divide the five preceding elements from the laſt letter M, and to an-

(22) *Sag. di Diſſertaz. Accademich. di Corton.* Tom. IV p. 133, 139.

nounce

nounce M the initial letter of the prenomen of our (23) Safinii's, or Safinius's, father. I would rather account for the ſmall point here mentioned in this manner than ſuppoſe it an accent, as Sig. (24) Olivieri does, ſince we find ſomething ſimilar to this on (25) Papius Mutilus's coins; ſuch a point being clearly viſible (26) there, between the element C, the initial letter of the prenomen of Mutilus's father and the word Paapi, that general's name. This obſervation will, perhaps, be allowed to bring no ſmall acceſſion of ſtrength to the interpretation of the word SAFINIM here laid down, and, in its turn, to receive no inconſiderable ſupport from that interpretation.

After maturely weighing the reaſons above alledged, I would flatter myſelf, that the Royal Society will adopt the preceding interpretation, as much eaſier, more ſimple and natural, and even more conſonant to the genius of ſuch Samnite coins as that here explained, than any of thoſe different (27) ones that have hitherto appeared, though propoſed by four of the greateſt (28) antiquaries this age, or indeed any other age, has produced. To that moſt illuſtrious body, therefore, I beg leave to ſubmit, with the utmoſt de-

(23) *Philoſoph. Tranſact.* Vol. LII. P. i. TAB. I. fig. 1. p. 34, 35.
(24) *Sag. di Diſſertaz. Accademich. di Corton.* ubi ſup. p. 139.
(25) *Philoſoph. Tranſact.* Vol. LII. P. i. TAB. I. p. 28. *Sag. di Diſſertaz. di Corton.* ubi ſup.
(26) *Philoſoph. Tranſact.* & *Sag. di Diſſertaz. di Corton. ubi ſup.* p. 133.
(27) Scip. Maff. ap. Annib. degl. Abat. Olivier. in *Sag. di Diſſertaz. di Corton.* Tom. IV. p. 142, 146, 147. Jo. Bapt. Paſſer, ibid. Olivier. ibid. Peller. ubi ſup. p. 11, 12, 13.
(28) Iidem ibid.

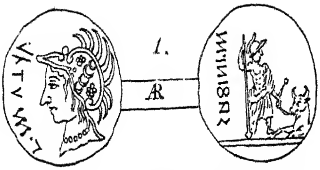
ference, what has been here advanced; as well knowing, that their determination of the point in queſtion will be received as deciſive by the whole learned world.

II.

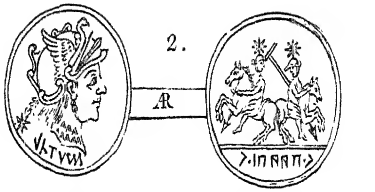
I find in M. Pellerin's third Supplement, publiſhed in the year 1767, a denarius (ſee Tab. XVIII. n. 6.), attributed by that very learned and ingenious gentleman to the city of Corfinium (29), the capital of the Peligni, where the deputies aſſembled, in order to regulate the operations of the war entered upon againſt the Romans, by the confederated Italian ſtates, towards the decline of the ſeventh century of Rome. This notion, which, I believe, will not be conteſted by the learned, he founds upon the appearance of the letter c (30) on the

(29) Peller. *Troiſiéme Supplément aux ſix volumes de recueils des medailles de rois, de villes, &c. publies en* 1762, 1763, & 1764, à Paris, 1767. p. 78—81. Pl. III. p. 90.

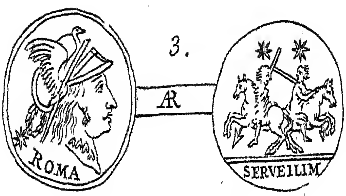
(30) Peller. ubi ſup. That Etruſcan coins are ſometimes adorned with the initial letters of the names of towns, is a point that will be allowed by every one moderately verſed in this branch of literature. I have one of theſe, with the Etruſcan letter 7 (ſee Tab. XVIII. n. 7.), and the uncial mark, or ſingle globule, upon it. On one ſide it exhibits an anchor, and on the reverſe a wheel. I received this piece from an Italian gentleman, and have ſome reaſon to believe it might have been found in Tuſcany. As therefore the anchor indicates it to have been ſtruck in a maritime town, it may perhaps not improperly be attributed to Coſa, or Coſſa, an antient and famous city of Etruria, ſeated near Telamon and Populonia, at a ſmall diſtance from the ſea, according to Strabo. This appears the more probable, as Etruſcan coins of Telamon and Populonia have been actually diſcovered, and communicated to the learned world. Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 428. Tab. cxcvi, cxcvii. Florentiæ, 1737. *Una Let-*



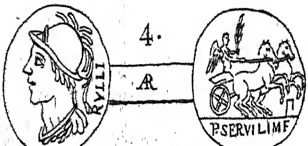
Sag. di Dissert. di Corton. Tom. IV. *p. 133.*



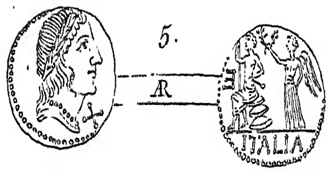
Sag. di Dissert. di Corton. Tom. IV. *p. 133.*



Sag. di Dissert. di Corton. Tom. IV. *p. 133.*



Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.



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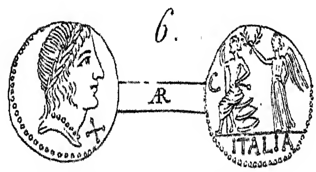
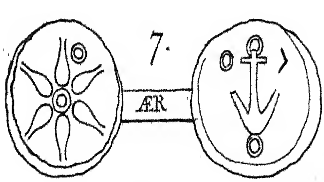


Peller. III *Supplem. p. 90.*



Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.

the reverſe; which he takes, with great reaſon, to be the initial letter of the word Corfinium, the name of that town. The coin of which I now ſend you a ſhort account (ſee TAB. XVIII. n. 5.) agrees with that denarius in every particular, but the letter on the reverſe; which is E, not C. But this is ſo far from overturning M. Pellerin's notion, that it will, at leaſt in my opinion, ſtrongly ſupport, if not entirely confirm, it; which will, if I am not greatly miſtaken, from what immediately follows, more clearly appear.

That theſe coins, and others ſimilar to them, firſt appeared about the time of the Social war, muſt be allowed extremely probable, from the ſymbols on the (31) reverſe which moſt of them exhibit, and has been obſerved by one of the greateſt Etruſcan (32) antiquaries of the preſent age. The letter E, on the reverſe of my denarius, has a Samnite, Samnite-Etruſcan, or oriental, direction, from the right hand to the left; which will, I conceive, notwithſtanding the Roman letters in the exergue, ſufficiently announce it a Samnite, or Samnite-Etruſcan, coin. This alſo will, in ſome meaſure, be evinced by the character itſelf; which more reſembles the antient Etruſcan form of E, than

tera del Sig. Olivier. al Sig. Abate Barthelemy, &c. p. 41, 42, 43. TAB. IV. n. 3, 4. In Peſaro, 1757. *Sig. Canonic. Mazocch. ſopra l'Origin. de' Tirren.* TAB. II. n. 15, 16. *in Sag. di Diſſertaz. Accademich. di Corton.* Tom. III. p. 61, 62. In Roma, 1741. *Strab. Geogr.* L. V. p. 156.

(31) *Diſſertaz. di* Annibale degli Abati Olivieri *ſopra due medaglie Sannitiche* in *Sag. di diſſertaz. Accademich. pub. let. nel. nobil. Accadem. Etruſc. dell' antichiſſ. cit. di Cortona.* Tom. II. p. 57. In Roma, 1738.

(32) Annib. degl. Abat. Olivieri, ubi ſup.

 the

the later Roman, or Latin, figure of that element. Now the old name of the city, to which both the medals here mentioned may, as I apprehend, be aſſigned, was Corfinium, and the new one, given it by the confederated Italian ſtates, Italica, as we learn from (33) Strabo. As the Samnites therefore and old Romans are known to have uſed E ſometimes for I, the element E, on the reverſe of my medal, may very well be ſuppoſed to have been the initial letter of the word Etalica, for Italica, the new name mentioned by Strabo (34). That the Samnites ſometimes uſed E for I, we may infer from the word EMBRATVR, for IMPERATOR (35), on ſome of Papius Mutilus's Samnite coins, to omit others that might, with equal facility, be produced; and that the antient Romans likewiſe did, not infrequently, the ſame thing, from the authors (36) here referred to, is indiſputably clear. From whence we may, as I apprehend, fairly collect, that M. Pellerin's denarius was ſtruck about the time the league was formed, or concluded, in commemoration of it; and mine after the commencement of the war, which was the immediate conſequence of that league. From what has been remarked, it is abundantly clear, that the pieces here mentioned not only receive a conſiderable ſhare of light from Strabo, but likewiſe evidently ſupport, in the point before us, the authority of that celebrated antient writer. Farther, as neither of theſe

(33) Strab. *Geograph.* lib. V. p. 241. Lutetiæ Pariſior. 1620.
(34) Strab. ubi ſup.
(35) Annib. degl. Abat. Olivieri, ubi ſup. p. 61.
(36) Cic. *de Orat.* lib. III. c. 12. Varro *de Ling. Latin.* lib. V. Quintilian. lib. V. c. 6. Aul. Gell. *Noct. Attic.* lib. X. c. 24.

pieces

pieces was ever publiſhed, I believe, before the publication of M. Pellerin's third Supplement, they ought both to paſs for a ſort of inedited coins; and, as ſuch, will have a place aſſigned them in the cabinets of the great, the curious, and the learned; which will, I hope, be deemed a ſufficient apology for the trouble given, in the latter part of this paper, by,

GOOD SIR,

Your moſt obliged,

and moſt obedient,

humble ſervant,

Chriſt-Church, Oxon.
July 5, 1769.

John Swinton.

 LXI. *Obſer-*

LXI. *Obſervation of the Tranſit of* Venus, *on* June 3, 1769. *In a Letter from* John Leeds, *Eſquire, Surveyor General of the Province of* Maryland, *to* John Bevis, *M. D. F. R. S.*

Talbot County, in Maryland,
June 17, 1769.

SIR,

Read Dec. 21, 1769.

AS you will, I believe, have but few accounts from Maryland of the tranſit of Venus, I take the freedom to ſend you this.

Having no other inſtruments to obſerve with but a pocket watch and a reflecting teleſcope about twenty inches long, of Sterrup's make, on the third inſtant, when the Sun was on the meridian, I ſet my watch to 12, and at half an hour paſt one began to obſerve, keeping my eye to that part of the Sun's limb a little north of the vertex, where I expected Venus to come on; 2^h $10'\frac{1}{2}$ I perceived a ſmall dent in the Sun's limb; 2^h $25'\frac{3}{4}$ Venus was totally within, ſo that the upper edge of the Sun and Venus ſeemed to touch. Being no better fitted for this buſineſs, I can give you no better account. I obſerved (and indeed from the calculation I expected it), that the

Planet

Planet entered deeper into the Sun's difc than that fcheme in Lowthorp's Abridgment of the Philofophical Tranfactions, Vol. I. Plate IV. fig. 155, feems to fhew.

My fituation is, lat. 38° 45′, under a meridian, as near as I can guefs, ten miles eaft of Annapolis, our chief town or city; and about twelve miles weft of Cape Henry, at the mouth of Chefopeak Bay, as laid down by Meffieurs Fry and Jefferfon in their map of Virginia and Maryland. I do not know that what I have here mentioned can be of any ufe; but, if it be impertinent, you will be pleafed to excufe it.

I am, SIR,

Your very humble fervant,

J. Leeds.

LXII. *Experiments to prove that the Luminousness of the Sea arises from the Putrefaction of its Animal Substances.* By John Canton, *M. A. and F. R. S.*

Read Dec. 21, 1769.

I SHALL not enter into the consideration of the several opinions of philosophers concerning the luminous appearance of the sea, as not one of them, that I know of, has been well supported; but I shall immediately relate a few experiments, which any person may very easily make, and which, I think, will be allowed to point out the true cause of that appearance, when compared with the descriptions given of it, by those who have accurately observed it.

Experiment I.

Into a gallon of sea-water, in a pan about 14 inches in diameter, I put a small fresh whiting, June 14, 1768, in the evening; and took notice that neither the whiting, nor the water when agitated, gave any light. A Fahrenheit's thermometer in the cellar, where the pan was placed, stood at 54 degrees. The 15th, at night, that part of the fish which was even with the surface of the water was luminous, but the water itself was dark. I drew the end of a stick through

through the water, from one side of the pan to the other, and the water appeared luminous behind the stick all the way, but gave light only where it was disturbed. When all the water was stirred, the whole became luminous, and appeared like milk; giving a considerable degree of light to the sides of the pan that contained it; and continued to do so for some time after it was at rest. The water was most luminous when the fish had been in it about 28 hours, but would not give any light by being stirred, after it had been in it three days.

Experiment II.

I put a gallon of fresh water into one pan, and a gallon of sea-water into another, and also into each pan a fresh herring of about three ounces. The next night the whole surface of the sea-water was luminous without being stirred, but much more so when put in motion; and the upper part of the herring, which lay considerably below the surface of the water was very bright. The fresh water was quite dark, as was also the fish that was in it. There were several very bright luminous spots on different parts of the surface of the sea-water; and the whole, when viewed by the light of a candle, seemed covered with a greasy scum. The third night, the light of the sea-water while at rest was very little, if at all, less than before; and when stirred, its light was so great, as to discover the time by a watch; and the fish in it appeared as a dark substance. After this, its light was evidently decreasing, but was not quite gone before the seventh night. The fresh water, and fish

in

in it, were perfectly dark during the whole time. The thermometer was generally above 60.

Experiment III.

Into a gallon of fresh water I put common or sea-salt, till I found by an hydrometer it was of the same specific gravity with the sea-water. In another gallon of fresh water I dissolved two pounds of salt: and into each of these waters I put a small fresh herring. The next evening the whole surface of the artificial sea-water was luminous without being stirred, but gave much more light when it was disturbed. It appeared exactly like the real sea-water in the preceding experiment, and its light lasted about the same time, and went off in the same manner*. The other water, which was almost as salt as it could be made, never gave any light. The herring, which was taken out of it the seventh night, and washed from its salt, was found firm and sweet; but the other herring was very soft and putrid; much more so than that which had been kept as long in the fresh water of the last experiment. If a herring, in warm weather, be put into ten gallons of artificial sea-water instead of one, the water will still become luminous, but its light will not be so strong.

N. B. The artificial sea-water may be made without the use of an hydrometer, by the proportion of

* Several river-fish, as the bleak, the dace, the carp, the tench, and the eel, were kept in artificial sea-water to putrefy, without producing any light that I could perceive: but a piece of a carp made the water very luminous, though the outside, or scaly part of it, did not shine at all.

four

four ounces avoirdupois of ſalt, to ſeven pints of water, wine meaſure.

From the ſecond and third experiments it is evident, that the quantity of ſalt contained in ſea-water haſtens putrefaction; as the fiſh that had been kept in water of that degree of ſaltneſs was found to be much more putrid than that which had been kept the ſame time in freſh water. This unexpected property of ſea-ſalt was diſcovered by Sir John Pringle, in the year 1750, and publiſhed in the XLVIth volume of the Philoſophical Tranſactions, with many very curious and uſeful experiments on ſubſtances reſiſting putrefaction; but the greateſt quantity of ſalt there mentioned, is leſs than what is found in ſea-water: it is probable, therefore, that if the ſea were leſs ſalt, it would be more luminous. And here it may be worth remarking, that, though the greateſt ſummer heat is well known to promote putrefaction, yet 20 degrees more than that of the human blood ſeem to hinder it: for, putting a very ſmall piece of a luminous fiſh into a thin glaſs ball, I found that water of the heat of 118 degrees would deſtroy its luminouſneſs in leſs than half a minute; which, on taking it out of the water, it would begin to recover in about ten ſeconds, but was never after ſo bright as before.

I ſhall now only add to theſe experiments the moſt circumſtantial accounts I can find of the ſea's luminous appearance. The Honourable Robert Boyle, in the third volume and 91ſt page, of Doctor Birch's edition of his works, ſays, " When I remem-

" ber how many queſtions I have aſked navigators
" about the luminouſneſs of the ſea; and how in
" ſome places the ſea is wont to ſhine in the night
" as far as the eye can reach; at other times and
" places, only when the waves daſh againſt the veſ-
" ſel, or the oars ſtrike and cleave the water; how
" ſome ſeas ſhine often, and others have not been ob-
" ſerved to ſhine; how in ſome places the ſea has
" been taken notice of, to ſhine when ſuch and ſuch
" winds blow, whereas in other ſeas the obſervation
" holds not; and in the ſame tract of ſea, within a
" narrow compaſs, one part of the water will be lu-
" minous, whilſt the other ſhines not at all: when,
" I ſay, I remember how many of theſe odd phæ-
" nomena, belonging to thoſe great maſſes of liquor,
" I have been told of by very credible eye-witneſſes,
" I am tempted to ſuſpect, that ſome coſmical law
" or cuſtom of the terreſtrial globe, or, at leaſt, of
" the planetary vortex, may have a conſiderable
" agency in the production of theſe effects."

Father Bourzes has given a ſtill more particular account of the luminous appearance of the ſea; part of which I have extracted from the third edition of Jones's Abridgment of the Philoſophical Tranſactions, Vol. V. Part ii. p. 213. " When the ſhip ran apace,
" we often obſerved a great light in the wake of the
" ſhip, or the water that is broken and divided by the
" ſhip in its paſſage. This light was not always
" equal; ſome days it was very little, others not at
" all; ſometimes brighter, others fainter; ſome-
" times it was very vivid, and at other times nothing
" was to be ſeen. As to its brightneſs, I could
" eaſily

"easily read by it, though I was nine or ten feet "above it from the surface of the water; as I did "particularly on the 12th of June, and the 10th of "July, 1704. But I could read only the title of my "book, which was in large letters. As to the ex- "tent of this light, sometimes all the wake ap- "peared luminous to thirty or forty feet distant "from the ship; but the light was very faint at any "considerable distance. Some days one might easily "distinguish in the wake such particles as were lu- "minous from those that were not: at other times "there was no difference. The wake seemed then "like a river of milk, and was very pleasant to "look on. At such times as we could distinguish "the bright parts from the others, we observed "that they were not all of the same figure. Some "of them appeared like points of light; others "almost as large as stars, as they appeared to the "naked eye. We saw some that looked like "globules of a line or two in diameter; and others "like globes as big as one's head. It is not "always that this light appears, though the sea "be in great motion; nor does it always hap- "pen when the ship sails fastest: neither is it "the simple beating of the waves against one an- "other, that produces this brightness, as far as I "could perceive. But I have observed, that the "beating of the waves against the shore has some- "times produced it in great plenty; and on the "coast of Brazil the shore was one night so very "bright, that it appeared as if it had been all on "fire.

 The

"The production of this light depends very
" much on the quality of the water; and, if I
" am not deceived, generally speaking, I may af-
" fert, other circumftances being equal, that the
" light is largeft when the water is fatteft, and fulleft
" of foam; for, in the main fea the water is not
" every where equally pure; and fometimes if one
" dips linen into the fea, it is clammy when it is
" drawn up again. And I have often obferved,
" that when the wake of the fhip was brighteft,
" the water was more fat and glutinous; and linen
" moiftened with it, produced a great deal of light,
" if it were ftirred or moved brifkly. Befides, in
" failing over fome places of the fea, we find a
" matter or fubftance of different colours, fome-
" times red, fometimes yellow. In looking at it,
" one would think it was faw-duft: our failors fay
" it is the fpawn, or feed of whales. What it is,
" is not certain; but when we draw up water, in
" paffing over thefe places, it is always vifcous and
" glutinous. Our mariners alfo fay, that there are
" a great many heaps or banks of this fpawn in the
" north; and that fometimes in the night they ap-
" pear all over of a bright light, without being
" put in motion by any veffel or fifh paffing by
" them.

" But, to confirm farther what I fay, videlicet,
" that the water, the more glutinous it is, the
" more it is difpofed to become luminous; I fhall
" add one particular which I faw myfelf. One day
" we took in our fhip a fifh, which fome thought
" was a boneta. The infide of the mouth of the

" fifh

" fiſh appeared in the night like a burning coal; " ſo that, without any other light, I could read by " it the ſame characters that I read by the light " in the wake of the ſhip. Its mouth being full " of a viſcous humour, we rubbed a piece of wood " with it, which immediately became all over lu- " minous; but, as ſoon as the moiſture was dried " up, the light was extinguiſhed.

" I leave it to be examined whether all theſe " particulars can be explained by the ſyſtem of ſuch " as aſſert, that the principle of this light conſiſts " in the motion of a ſubtle matter, or globules, " cauſed by a violent agitation of different kinds " of ſalts."

LXIV. *A Series of Astronomical Observations made at the Observatory of the Marine at* Paris, *to wit,* 1°. *Observations of* Jupiter's *Satellites in the Years* 1767 *and* 1768. 2°. *Observations on the Shadows of* Jupiter's *Satellites.* 3°. *On the Variation of the Belts on the Disc of that Planet.* 4°. *Observation of a Spot on the Disc of the 3d Satellite.* 5°. *Observation of the Belts of* Saturn. 6°. *Observation of the* Moon's *Passage over the* Pleiades, *in* 1767. 7°. *Observation of a partial Eclipse of the* Moon, January 3, *and of a total one,* December 23, 1768. 8°. *Observations of Two* Auroræ Boreales, August 6, *and* December 5, *of the same Year. By* M. Messier, *Astronomer of the Marine, F. R. S. and of the Academies of* Holland *and* Italy.

Observations of Eclipses of Jupiter's 4 Satellites, made at Paris, in the Observatory of the Marine, in the Year 1767, with an excellent Gregorian Telescope of 30 Inches Focus, the great Speculum 6 Inches Diameter, and the magnifying Power 104 Times.

1767	True time h ′ ″	
Jan. 25	11 18 17	I. 4. Sky serene. ♃ well defined, the satellite 12′ losing its light, during the two last of which it was extremely small. ♃ 28° high in the east. Good observation.

1767

1767	True time h	′	″	
Jan. 25	15	9	57	E 4. Sky ſerene. ♃ 47° high in the weſt. Good obſervation. In both I kept ♃ without the field of the teleſcope, to view the ſatellite the better.
26	15	34	34	I. 1. Clear ſky. ♃ well defined, 46° high in the weſt. Good obſervation.
29	10	18	7	I. 2. Serene. ♃ well defined, 22° high in the eaſt. Good obſervation.
Febr. 2	17	27	13	I. 1. ♃ well defined, 31° high in the weſt. The ſatellite extremely ſmall for a minute. Good obſervation.
19	17	57	56	I. 2. Sky ſerene round ♃, being 16° high weſtward. Good obſervation.
March 8	8	31	35	I. 1. Clear ſky. The ſatellite diſappeared touching the diſc of ♃. Doubtful to ſome ſeconds, the oppoſition being this night at $10^h\ 49'\ 4''$. At $10^h\ 29'\ 4''$ the ſatellite begins to appear at ♃'s eaſtern limb; at $10^h\ 51'\ 34''$ half out; at $10^h\ 51'\ 34''$ quite ſeparated from ♃'s limb.
20	7	7	26	E. 2. Clear ſky about ♃. The ſatellite emerged at $\frac{1}{3}$ of the Planet's diameter. ♃ 25° high eaſtward. Good obſervation.
April 9	7	29	50	E. 1. Serene; but the obſervation doubtful to ſome ſeconds, from the proximity of ♃ to the Moon and the horizon.
10	14	58	18	E. 2. Clear ſky; but ♃ near the horizon and the Moon. Obſervation doubtful to 5 or 6 ſeconds. The ſatellite emerged at $\frac{1}{3}$ of a diameter from the Planet. ♃ 15° high weſtward.
28	9	32	10	E. 2. Clear ſky. ♃ well defined. The ſatellite emerged $\frac{3}{4}$ of a diam. from ♃, which was 47° high. Good obſ.

1767	True time h ′ ″	
April 29	10 13 29	E. 3. Clear ſky. ♃ well defined. The ſatellite already emerged, perhaps for ¾ of a minute; it recovered not its full light till 4′ after. The emerſion was a diameter from ♃.
May 5	12 9 39	E. 2. Clear ſky. ♃ well defined. Satellite emerged at more than a diameter from ♃, being then 25° high weſtward. Good obſervation.
6	11 17 37	I. 3. Sky much clouded. ♃ ſeen through thin clouds; but I think the obſervation may be depended on to 10 or 12 ſeconds.
Dec. 21	18 34 32	I. 3. Clear ſky. ♃ well defined. Satellite entered the ſhadow 1 diameter from ♃, after having been 2′ extremely ſmall. Good obſervation.
23	14 43 47	I. 1. Clear ſky; but ♃ near the horizon, and ill defined. However, I eſteem the obſervation a good one. The ſatellite emerged ⅗ of a diameter from ♃, being then 12° above the horizon.
March 15	12 41 48	E. 1. At Colombes, near Paris, 20⅓ ſeconds of time weſt of the Royal obſervatory. The ſatellite out of the ſhadow, with a ten feet refractor of Dollond, magnifying 120 times. Good obſervation.
April 16	9 27 3	E. 1. At Colombes; ſerene ſky; the ſatellite emerged at ¼ diameter from ♃; with a Dollond's refractor of ten feet.
May 30	9 14 47	E. 2. At Calais; ſky ſerene round ♃, with a 3½ feet refractor of Dollond, with a triple object-glaſs. The ſatellite emerged 1 diameter from the Planet, a little above the 4th ſatellite. Good obſervation.

1767	True time h ′ ″	
June 1	9 51 40	E. 1. At Calais; ſky ſerene; ♃ well defined. The ſatellite emerged at the diſtance of ½ a diameter. Same refractor. Good obſervation.
8	11 24 4	I. 4. At Dunkirk; ſerene ſky; but ♃ near the horizon; the belts, however, appeared plain, and I think it a good obſervation. The ſatellite was more than 6′ loſing its light. Dollond's 3½ feet refractor.
	11 47 54	E. 1. At Dunkirk; ſerene ſky; but ♃ very near the horizon, and ill defined. Doubtful obſervation.
1767		Obſervations on the ſhadow of Jupiter's ſatellites, and the variations of his belts, at the Obſervatory of the Marine, with a Gregorian reflector, of 30 inches focus, magnifying 104 times.
Febr. 19	12 58 46	The ſhadow of the firſt ſatellite appeared entirely on ♃'s diſc, and proceeded along the upper part of the middle belt.
	13 18 3	The ſatellite itſelf entered half way on ♃'s diſc, following its ſhadow on the ſame belt.
	14 1 7	The ſhadow of the fourth entirely entered on ♃'s diſk, and running along the lower edge of ♃'s upper belt.
	15 3 57	The ſhadow of the firſt ſatellite in internal contact with ♃'s limb.
	15 7 27	The ſhadow of the firſt quite off ♃'s limb.
	15 25 23	The firſt beginning to go off the diſc.
	15 29 53	The firſt gone off, but ſtill in external contact with the limb.
	15 29 53	The ſhadow of the fourth ſeems advanced one half of its path over the diſc.
	16 29 43	The ſhadow of the fourth ſeems to be two of its own diameters from ♃'s limb.

1767	True time h ′ ″	
Febr. 19	16 42 41	The ſhadow of the fourth, at leaſt, one of its own diameters from ♃'s limb.
	16 54 39	The ſhadow of the fourth looks oval, and touches ♃'s limb.
	17 5 35	The ſhadow of the fourth quite off ♃'s weſtern limb.
	17 5 35	The fourth ſatellite almoſt touches ♃'s eaſtern limb.
	17 13 34	The fourth ſatellite entering on ♃'s diſc, and forming an indentation on his limb.
	17 35 32	The fourth ſatellite quite entered, but not ſo viſible on the diſc as the firſt.

The ſhadow of the fourth ſeemed larger than that of the firſt; it was alſo more perceptible; owing, perhaps, to the ſhadow of the firſt running along the upper ſide of the middle belt, which was darker and more conſpicuous than the upper belt, where the ſhadow of the fourth was. It was an eaſy matter to form a judgment of their differences, as the two ſhadows were at the ſame time ſeen on ♃'s diſc. At $6\frac{3}{4}^h$ in the morning the ſky became clouded, ſo that the egreſs of the fourth from the diſc could not be obſerved.

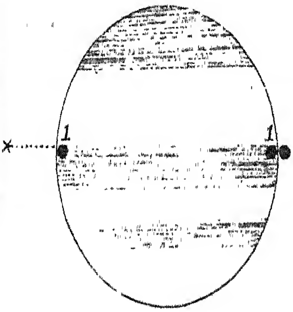
The figure n° 1 (Tab. XIX) repreſents ♃'s diſc at the moment of the entry of the ſhadow of the firſt ſatellite; the upper part was ſhaded, but leſs ſenſibly than the middle belt, which was blackiſh, and of a darker hue in ſome parts than in others. Below this middle belt, another was viſible, which terminated at about two-thirds of ♃'s diameter. It was narrow, but as diſtinguiſhable as the middle belt.

The fig. n° 2 repreſents the ſhadow of the fourth ſatellite, which paſſed along the upper belt leſs diſtinct than the middle one. At $3^h\ 11''$ the lower narrow belt was not to be ſeen; the middle belt was alſo altered, being of various ſhades, and different from that repreſented in fig. 1. at the time of the immerſion of the ſhadow of the firſt ſatellite.

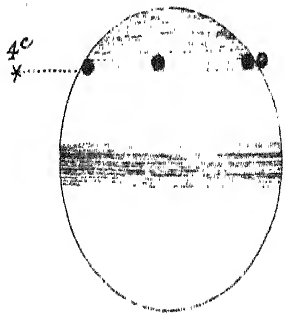
The fig. n° 3 repreſents the ſhadows of the firſt and fourth ſatellites, viewed at the ſame time.

The fig. n° 4 repreſents ♃'s belts: the upper belt was the ſame as in fig. n° 1; the middle belt was likewiſe the ſame: the

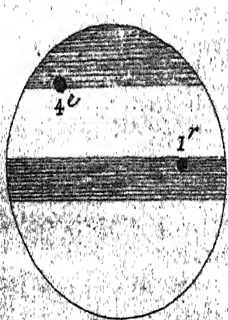
I.



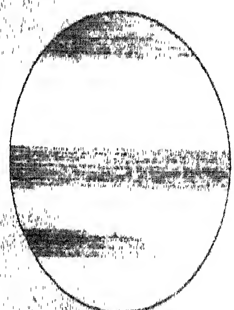
II.



III.



IV.



the narrow belt, which had disappeared, appeared again on the eastern side. At $4^h\ 30'$ it was advanced $\frac{1}{4}$ of the diameter. At $5^h\ 58'$ it extended almost from one limb to the other, as narrow and sensible as in fig. n° 1.

The 6th of September, 1760, having computed the ingress of the third satellite on ♃'s disc, I viewed the Planet with a Gregorian reflector of 30 inches focus, magnifying 104 times. At $7^h\ 4''$ I perceived at the center of the Planet, on the lower belt, a black spot, pretty round, and nearly of the size of the shadow of the first satellite. I guessed this spot to be the shadow of the third. I observed its progress, and being got on $\frac{2}{3}$ of ♃'s disc, at $8^h\ 13'$ I perceived the shadow of the third just entered on ♃'s eastern limb, and was larger than that I observed before, which made me to imagine, that the first shadow might be a spot on the very disc of the third satellite. I went on with my observations, and found, that the nearer this shadow approached the western limb of ♃, the more it was diminished in size, and I lost sight of it before it had got to the Planet's limb. Lastly, at $9^h\ 26'$, the third satellite was half emerged, and formed an indentation on the disc. I was then well satisfied, that the observed spot was on the disc of the third satellite; and I took notice, that this satellite, when quite emerged, was not so luminous as usual.

The 28th of March, 1766, having viewed Saturn with the same achromatic reflector of 10 feet 7 inches focus, I perceived on his globe two darkish belts; they were indeed extremely faint, and difficult to be discerned, directed, however, in a right line parallel to the longest diameter of Saturn's ring.

The several observations here recited are extremely nice; and it were to be wished that astronomers, concerned in observations, might be accommodated with achromatic telescopes, of the most perfect construction; as such are the only instruments whereby a great knowledge of the celestial bodies can be acquired, for the improvement and perfection of astronomy.

Obſervations of the Moon's Tranſit over the Pleiades, in 1767, at the Obſervatory of the Marine.

1767	True time h ′ ″	
Sept. 12	14 11 42	Imm. of ✱ *b*, Electra, at the illuminated limb of the Moon.
	15 0 49½	Imm. of ✱ *d*, Merope.
	15 6 8	Imm. of ✱ *e*, Maia, near the limb, as it were ſhaving it.
	15 14 56	Emerſ. of ✱ *s*, Celeno, doubtful to 5 or 6 ſeconds, from thin clouds.
	15 24 49	Emerſ. of ✱ *b*, Electra. Good.
	15 31 11	Emerſ. of ✱ *η*, Alcyone. Good.
	15 47 39	Emerſ. of ✱ *e*, Maia. Good to a ſecond.
	15 54 31	Emerſ. of ✱ *d*, Merope. Doubtful. Looking into the teleſcope, was viſible; though but a ſmall time emerged.
	16 51 11	The Moon clear of clouds, *η* appeared; it had emerged a few minutes.
	17 2 10	Conjunction of ✱ *f* Atlas. It paſſed very near the Moon's limb, being only 10 parts of the micrometer from it, equal to 9 ſeconds.
Nov. 6	11 9 50	Imm. of ✱ *d*, Merope. Doubtful to 2 or three ſeconds, from the great light of the Moon.
	11 57 57½	Imm. of ✱ *η*, Alcyone. Good to a ſecond.
	12 11 55½	Emerſ. of ✱ *d*, Merope. Good to a ſecond.
		The ✱ *s* was effaced by the light of the Moon.
	12 31 59	Imm. of ✱ *f*, Atlas. Good.
	12 45 50	Emerſ. of ✱ *η*, Alcyone. It was already out, perhaps half a minute.
	13 42 25½	Emerſ. of ✱ *h*, Pleione. The immerſion could not be obſerved, for the great light of the Moon. The emerſion good to a ſecond.

1767	True time h ′ ″	
Nov. 6	13 43 28½	Emerſ. of ✳ *f*, Atlas. Good to half a ſecond.
Dec. 31	4 32 57	Imm. of ✳ *d*, Merope.
	4 40 18½	Emerſ. of ✳ *d*, Merope.
	5 14 0	Conjunction of ✳ *η*, Alcyone 280 parts of the micrometer = 4′ 5″ from the Moon's upper limb.
	5 27 58	Imm. of ✳ *f*, Atlas, into the obſcure limb of the Moon. Doubtful to 2″.
	6 20 20	Emerſ. of ✳ *f*, Atlas, from the illumined limb of the Moon againſt Cleomedes. Good. ✳ *b* could not be obſerved for moon-light.

Obſervations of the eclipſes of Jupiter's ſatellites, made at the Obſervatory of the Marine at Paris, in the year 1768, with a Gregorian reflector, of 30 inches focus, magnifying 104 times.

1768	True time h ′ ″	
Jan. 31	12 53 22	Imm. 1. Sky ſerene. ♃ well defined. Good obſervation.
Feb. 2	18 4 36	Imm. 3. Sky ſerene. The ſatellite extremely ſmall for two minutes.
14	16 39 24	Clouds covered ♃. The firſt ſatellite had then loſt much of its light: 28″ after ♃ re-appeared, but the firſt ſatellite was no longer ſeen. I put the immerſion at $16^h\ 39'\ 50''$.
March 1	14 57 56½	Imm. 1. Sky ſerene. The Moon above the horizon, which did no great harm. ♃ was well defined; the ſatellite very ſmall for 45″, it entered the ſhadow at ⅓ of a diameter from ♃. ♃ 35° high weſtward. Good obſervation.
10	11 22 15	Imm. 1. Sky ſerene. ♃ well defined. For 30″ the ſatellite was very ſmall. It entered the ſhadow at ¼ diam. from ♃. ♃ 26° high eaſt. Good obſerv.

1767

1768.	True time h ′ ″	
April 3	0 55 34	Imm. 2. Sky ſerene round ♃, which was well defined. The ſatellite entered the ſhadow near ♃'s limb. I eſteem it a good obſervation.
27	8 35 11½	Emerſ. 1. Sky ſerene. ♃ pretty well defined: the Moon, though near him, did not much incommode. The ſatellite emerged ⅛ diameter from ♃, then 32° high eaſtward. Good obſervation.
May 4	10 31 0	Emerſ. 1. Sky ſerene. ♃ well defined. Satellite emerged ⅕ diameter from ♃, then 36° high, having lately paſſed the meridian. Good obſervation.
5	13 7 51	Emerſ. 2. ♃, among interſtices of clouds, was not well defined; the belts not diſtinct. Satellite emerged ⅕ diameter from ♃, then 22° high. Pretty good obſervation.
11	12 26 15	Emerſ. 1. ♃ well defined. Good obſervation.
20	8 49 54	Emerſ. 1. Sky pretty clear, and ♃ well defined. With a Dollond's 5 feet refractor with a double object glaſs. Satellite emerged ⅓ diameter from ♃. The ſatellite very ſmall, and the obſervation good.
27	10 03 50	Imm. 3. Sky ſerene. ♃ well defined. Satellite continued very ſmall for 3′. It entered the ſhadow at ¼ diameter from ♃, then 32° high weſtward. Good obſervation.
	10 43 14	Emerſ. 1. Sky ſtill ſerene, and ♃ well defined. Satellite emerged ⅓ diameter from ♃, then 30° high weſtward. Good obſervation.
	12 5 42	♃ juſt free of clouds. The third ſatellite appears in nearly its full luſtre.
June 3	12 37 39	Emerſ. 1. Sky ſerene; but ♃ too near the horizon; and, being among vapours, ill defined. Satellite emerged

1767

1768	True time	
	h ′ ″	$\frac{1}{3}$ diameter from ♃, then 10° high. Good to a few seconds.
June 19	10 53 43	Emerf. 1. Sky partly clouded, but ♃ pretty bright at the time of obfervation. Satellite emerged $\frac{1}{3}$ diameter from ♃, then 17° high. Good to a few feconds.
July 5	9 8 5	Emerf. 1. Sky clear. ♃ well defined. The emerfion at $\frac{1}{3}$ diameter from ♃, 22° high weftward. A good obfervation.
28	9 20 20	Emerf. 1. Clear fky, but ♃ only 5° high weftward. The Planet ill defined, though the belts were plain enough. A doubtful obfervation.

Obfervation of a partial Eclipfe of the Moon, on the night between the 3d of January, 1768, at the Obfervatory of the Marine at Paris, with a four Feet Newtonian Reflector, magnifying 66 Times, armed with a Micrometer.

1768	True time h ′ ″	
Jan. 3	11 50 20	The Moon's centre paffed the meridian.
	12 8 38	θ. II. paffed.
		1° 27′ 41″, the difference between the altitude of the Moon's upper limb and that of the ftar, the ftar being lower.

1768. January 3. True time. h ′ ″	Uneclipfed part of the Moon. ′ ″	The part eclipfed. ′ ″	
15 14 40			The penumbra fenfible.
15 18 49			Very ftrong.
15 22 48			The eclipfe begins.
15 28 47			Immerfion of Tycho.
15 30 22	27 15	4 5	
15 36 46	25 52	5 28	
15 49 44	22 43	8 37	
15 54 43	22 8	9 12	

1768

1768. January 3. True time. h ′ ″	Uneclipsed part of the Moon. ′ ″	The part eclipsed. ′ ″	
16 0 12	20 46	10 34	
16 4 40	20 14	11 6	
16 9 34			Fracaſtorius enters the ſhadow.
16 10 40			The ſame entirely in the ſhadow.
16 10 40			Mare Nectaris enters the ſhadow.
16 12 39	19 24	11 56	
16 15 39	31 20		The Moon's diameter.
16 18 38	18 47	12 33	
16 21 38			Mare Nectaris half entered.
16 23 8	18 40	12 40	
16 24 38			Mare Nectaris totally in the ſhadow.
16 26 37	18 31	12 49	
16 26 37			Langrenus is entered into the ſhadow.
16 31 36	18 18	13 2	
16 35 35			Mare Imbrium nearly half entered.
16 38 34	18 20	13 0	
16 40 34	18 22	12 58	
16 46 34	18 38	12 42	
16 48 33	18 49	12 31	The Moon in a miſt, the ſhadow ill defined.
17 3 30	20 15	11 5	The Moon pretty clear, Mare Humorum got out of the ſhadow 7′ or 8′.
17 5 0			Mare Nubium clear of the ſhadow.
17 7 29	21 17	10 3	
17 12 28			Mare Nectaris begins to emerge.
17 14 28	22 12	9 8	
17 14 58			Tycho half out.
17 16 57			Mare Nectaris half out.
17 16 57	23 9	8 11	
17 17 27			Tycho clear of the ſhadow.
17 21 27	24 13	7 7	
17 25 26			Fracaſtorius out of the ſhadow.
17 26 26			Mare Nectaris out of the ſhadow.
17 27 56	25 18	6 2	
17 34 55	26 57	4 23	

1768. January 3. True time. h ′ ″	Parts enlightened. ′ ″	Parts eclipsed. ′ ″	
17 36 25			Mare Imbrium out of the shadow, which did not cover one half of it.
17 39 53	28 57	2 23	
17 43 23	29 42	1 38	
17 43 53			The Moon's limb begins to be visible.
17 45 53	29 59	1 21	
17 46 54			End of the eclipse.
17 51 22	31 32		Moon's diameter.
17 55 20			Much penumbra still left.
17 57 30			Now as before the eclipse began.
18 14 20			Now little or nothing sensible.

During the night of the eclipse the sky was greyish, and overspread with thin clouds; but the stars might be discerned. The shadow pretty well defined, and would have been much more if the sky had been clear. The cold was considerable all night long, the thermometer marking 11 degrees below 0, on Reaumur's scale.

Observation of the total Eclipse of the Moon, the 23d of December, 1768, in the Evening, at the same Observatory of the Marine, with the same Newtonian Reflector of 4½ feet. The Sky serene during the 23d Day, but some Clouds in the West during the Eclipse; in the East serene, with a small Mist, which was favourable; the Shadow well defined. I had no View of the Moon, at the Horizon, because of elevated Land Objects.

1768. Dec. 23. True time. h ′ ″	Parts enlightened. ′ ″	Parts eclipsed. ′ ″	
4 27 0			The Moon beginning to appear over the church of Nôtre Dame, seeming to the bare eye half way eclipsed. Tycho already some minutes emerged.

1768, December 23. True time. h ′ ″	Parts enlightened. ′ ″	Parts eclipsed. ′ ″	
4 35 36	15 4	17 29	
4 39 3	17 6	15 27	
4 41 0	18 16	14 17	
4 43 7	19 58	12 35	
4 44 30			Mare Serenitatis quits the ſhadow.
4 44 39			Dionyſius quits the ſhadow.
4 46 14	21 41	10 52	
4 47 54			Mare Serenitatis half out.
4 49 34	23 20	9 13	
4 49 48			Mare Tranquillitatis half out.
4 51 8			Mare Serenitatis quite out.
4 52 18	24 31	8 2	
4 54 18			Mare Tranquillitatis clear of the ſhadow.
4 55 26	26 40	5 53	
4 56 29			Mare Imbrium half out.
4 58 4			Mare Criſium half out.
4 59 7	28 57	3 36	
5 0 34			Mare Criſium and Mare Imbrium quite out.
5 2 10	30 52	1 41	
5 2 50			The Moon's limb beginning to be diſcernible.
5 3 36			The end of the ſhadow, or of the eclipſe.
5 4 6			Yet more certain.
5 5 6			Strong penumbra.
5 5 46			Yet very ſenſible.
5 6 45	32 32		The Moon's diameter.
5 8 45			The penumbra ſtill remains.
5 9 45	32 34		The Moon's diameter.
5 15 44			A ſmall matter of the penumbra ſtill remaining.

LXV. *Aſtrono-*

Read November 16, 1769.

LXV. *Astronomical Observations made by Order of the* Royal Society, *at* Prince of Wales's Fort, *on the North-West Coast of* Hudson's Bay. *By* William Wales *and* Joseph Dymond.

Mem. The Thermometer marked A was hung within the southern, or lower Observatory; in such a Part as we judged would be least affected by the Fire; close to, and with its Ball exactly of the same Height with, the Quicksilver in the Bason of the Barometer: That marked B was hung without Doors, on the north Side of the Observatory.
The Floor of the Observatory might be above 50 Feet above the Level of the Sea at Low-water Mark.

1768		Equal altitudes. Times by the clock.				Zenith distance.	Barometer.	Thermometers.		Phænomena and Circumstances.		
Septemb.		Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian. h ′ ″	° ′	Inches	A	B	Observer		
☿	14	17 15	19 20 54	24 33	23 49 18½	75 40	29,56	38½	38	W.	☉'s U. L.	easterly
		21 37	25 16	28 55							☉'s L. L.	
♃	15	15 59	4 12 18	8 37		75 40	29,61	46	42½	W.	☉'s L. L.	westerly
		20 24	16 44	13 5							☉'s U. L.	
☉	18	At noon wound up the clock.								D.		
☽	19	14 54	19 18 36	22 10	23 54 7	78 13	29,97	37	34	D.	☉'s U. L.	
		19 15	22 56	26 37							☉'s L. L.	easterly
		33 8	36 55	40 39		76 0	29,98	37	34	D.	☉'s U. L.	
		37 37	41 25	45 11							☉'s L. L.	
♂	20	9 36	4	2 7		76 0	30,07	49	44	D.	☉'s L. L.	
		14 3	4 10 19	6 32	23 55 00,-						☉'s U. L.	westerly
		28 1	24 16	20 37		78 13	30,07	49	44	D.	☉'s L. L.	
		32 19		25 1							☉'s U. L	
		36 56	19 40 44			76 0	30,06	38½	39	D.	☉'s U. L.	easterly
		41 26									☉'s L. L.	
☿	21					76 0	30,10	56	56	D.	☉'s L. L.	westerly
		4 33	4	12 3	23 55 57,-						☉'s U. L.	
		15 47	19 19 30	23 11		79 0	29,97	45	44	D.	☉'s U. L.	
		20 13	19	27 27							☉'s L. L.	
		40 42	19 44 34	48 22		76 0	29,96	46	45	D.	☉'s U. L.	easterly
		45 16	49 7	52 54							☉'s L. L.	
		0 47	20 4 44	8 41		73 40	29,95	47	47	D.	☉'s U. L.	
		5 27	9 28	13 25							☉'s L. L.	
♃	22	45 26	3	37 27		73 40	29,89	62	65	D.	☉'s L. L.	
		50 8	46 9	42 9							☉'s U. L.	
		5 38	4 1 42	58 3		76 0	29,89	62½	65	D.	☉'s L. L.	westerly
		10 2	6 18	2 32							☉'s U. L.	
		30 36	26 52	23 5		79 0	29,89	63	65	D.	☉'s L. I.	
		34 57	31 18	26 37							☉'s U. L.	

♂ Sept. 27 From the preceding obſervations, I have found that the clock is gaining 1′ 18″ per day on mean ſolar time, and in conſequence of that is now about 10′ 9″ too faſt: therefore at 20ʰ we ſtopped it, altered the pendulum to make it go ſlower, and ſet it to nearly mean time, W. W.

1768		Equal altitudes. Times by the clock.				Zenith diſtance	Baro-meter.	Thermo-meters.		Phænomena and Circum-ſtances.		
September		Lower Wire. ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Paſſed the Merdian. h ′ ″	° ′	Inches.	A	B	Ob-ſerver.		
☿	28	38 24	20 42 54	47 22	23 52 $16\frac{1}{2}$	71 40	29,96	$43\frac{1}{2}$	44	W.	☉'s U. L.	eaſterly
		43 46	48 20	42 55							☉'s L. L.	
♃	29	59 50	2 55 15	50 43		71 40	29,98	56	55	W.	☉'s L. L.	weſterly
		5 14	3 0 43	56 17							☉'s U. L.	
♀	30	47 22	19 51 18	55 15	23 53 $13\frac{1}{2}$	78 0	29,82	36	35	W.	☉'s U. L.	eaſterly
		52 7	56 4	0 1							☉'s L. L.	
		8 6	20 12 18			76 0					☉'s U. L.	
		13 6	17 17	21 27							☉'s L. L.	
♄ Oct.	1	.				76 0	29,72	$49\frac{1}{2}$	$50\frac{1}{2}$	W.	☉'s L. L.	weſterly
			3	40 24	23 53 $46\frac{1}{4}$						☉'s U. L.	
		53 19:	3 49 21	45 25		78 0					☉'s L. L.	
		58 4	54 9	50 14							☉'s U. L.	
		42 9	19 46 5	50 0		79 0	29,34	41	41	D.	☉'s U. L.	eaſterly
		46 52	50 49	54 44							☉'s L. L.	
		59 46	20 3 50	7 52		77 0	29,33	$41\frac{1}{2}$	41	D.	☉'s U. L.	
		4 36	8 43	12 45							☉'s L. L.	
☉	2	At noon wound up the clock.								D.		
		41 57	3 37 52	33 49		77 0	29,18	50	$50\frac{1}{2}$	D.	☉'s L. L.	weſterly
		46 48	42 44	38 43							☉'s U. L.	
			55 42	51 47		79 0	29,18	50	$51\frac{1}{2}$	D.	☉'s L. L.	
		4 20	4 0 26	56 33							☉'s U. L.	
☉	9	At noon wound up the clock.								W.		
♃	13	32 35	20 37 5			78 20	29,86	28	26	W.	☉'s U. L.	eaſterly
		37 54		46 55							☉'s L. L.	
		52 49	20 57 38			76 20	29,87	29	$26\frac{1}{2}$	W.	☉'s U. L.	
		58 32	21 3 26								☉'s L. L.	
♀	14	2 18	2 57 26		0 0 $53\frac{1}{4}$	76 20	29,90	41	37	W.	☉'s L. L.	weſterly
		8 2	3 3 15								☉'s U. L.	
		22 54	18 21	13 54		78 20					☉'s L. L.	
		28 14	23 47	19 21							☉'s U. L.	
☉	16	At noon wound up the clock.								D.		
		37 54	20 42 28	46 59		79 0	30,19	21	20	D.	☉'s U. L.	eaſterly
		43 24	47 57	52 30							☉'s L. L.	
☽	17		3 16 58	12 20	0 2 55,–	79 0	30,20	31	$28\frac{1}{2}$	D.	☉'s L. L.	weſterly
		27 2		17 57							☉'s U. L.	
☉	23	At noon wound up the clock.								W.		
♂	25	Stopped the Clock 24′ 11″ and ſcrewed down the ball of the pendulum $\frac{3}{4}$ of a turn, W. W.										
		39 31	20 44 28	49 30	23 44 $47\frac{1}{2}$	80 0	29,78	15	8	W.	☉'s U. L.	eaſterly
		45 28	50 24	55 44							☉'s L. L.	
		2 23	21 7 53:	13 32		78 0	29,77	15	$7\frac{1}{2}$	W.	☉'s U. L.	
		9 0	14 43	20 28							☉'s L. L.	

7

1768	Equal altitudes. Times by the clock.				Zenith distance	Barometer.	Thermometers.		Phænomena and Circumstances.	
October.	Lower Wire. ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	° ′	Inches	A	B	Observer.	
☿ 26	19 48	2 14 6			78 0	29,64	19	15	W.	☉'s L. L. westerly
	26 22:	20 53	15 20::							☉'s U. L.
	43 15	2 38 9	33 0:		80 0	29,63	18½	14½	W.	☉'s L. L. very hazy
	49 12	44 14	39 12							☉'s U. L
☉ Nov. 6	57 45	8 1 15	4 42	13 19 27¼	71 0	29,63	3	— 4½	W.	Aldebaran easterly
	8 2	11 32	14 59		69 40					
		18 27 21	30 51		69 40	29,56	— 3	—11	W.	Aldebaran westerly
	34 14	37 42	41 10		71 0					
☿ 16	37 55	8 41 35	45 12	12 44 3¾	61 20	29,60	+ 9	— 3	D.	Aldebaran easterly
	56 56	9 0 40	4 21		59 0					
	31 10	16 27 28	23 45		59 0	29,60	+ 1	— 8	D.	Aldebaran westerly
	50 14	46 32			61 20					
♃ 17	24 1	7 27 27	30 56	12 40 34″,-	70 20	29,73	— 1	— 9	D.	Aldebaran easterly
	36 53	40 25:	43 52		68 40					
		53 15	56 46		67 0					
	31 21	17 27 50	24 24		67 0	29,83	— 7	—12	D.	Aldebaran westerly
	44 15:	17 40 44	37 17		68 40					
	57 7	53 40:	50 13:		70 20					
♀ 18	1 52:	8 5 27	8 56	12 37 1,7	65 0	29,88		—14	D.	Aldeb. easterly, hazy
	12 11	7 8 38	5 6		65 0	29,90	— 3	—15	D.	Aldebaran westerly
♄ 19	2 9	9 6 17	10 43		57 0	29,85	— 9	— 6	D.	Aldebaran easterly
	17 31	21 30	25 27		55 20				D.	Ditto Ditto

☉ 27 At noon I went to wind up the clock, but found it had stopped at $6^h\ 48'$. I suppose it had been stopped by the cold last night, and therefore I kindled a fire to warm it before it was set a going. At about $6^h\ 58'$ by the alarm set the regulator a going and wound it up. J. D.

♂ Dec. 6	13 28	6 16 58	20 24	11 22 11,1	69 20	29 94	—11½	—23½	W.	Aldebaran easterly
	26 19	29 49	33 18:		67 40					
	18 3	16 14 32			67 40	29 98	—16	—26	W.	Aldebaran westerly
	30 56	27 24			69 20					
♃ 8	6 22	6 9 52	13 19	11 15 6⅓	69 20	30 21	— 2	—15	W.	Aldebaran easterly
	23 52	16 20 22	16 53		69 20	30 14	— 4½	—14	W.	Aldebaran westerly.
♄ 10		6 10 21	13 49	11 7 57,3	68 20	29 58	—10	—23½	W.	Aldebaran easterly
	17 11	6 20 41	24 9		67 0					
	58 43½	15 55 13			67 0	29 51	—23	—32	W.	Aldebaran westerly
	8 4	16 5 34	2 6		68 20					
☉ 11	3 25	6 6 55		11 4 29,8	68 20	29 50	—27	—37	D.	Aldebaran easterly
	16 21	19 51	23 20		66 40					
		15 49 9			66 40	29 39	—31	—42	D.	Aldebaran westerly
	5 38	16 2 3			68 20					

At 21^h I found that the regulator had stopped at $20^h\ 16'\ 3''$, notwithstanding the fire was very good, and by agreement with Mr. Wales, I let the fire go out, the stove being obliged to stand so near the side of the observatory that a little extraordinary fire would endanger the same, it having twice melted the lead at the back already; I also took of the weight off the regulator to ease it, and let it stand. J. D.

1769		Equal altitudes. Times by the clock.				Zenith distance	Barometer.	Thermometers.			Phænomena and Circumstances.
March.		Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	° ′	Inches	A	B	Observer	
☿	15	39 1 53 33	7 43 50 7 58 44	48 39 3 58	10 7 37¾	53 0 51 40	29,76	−22	−30	W.	Regulus easterly
		21 44 36 13	 12 31 25	 26 37		51 40 53 0	29,68	−28	−34	W.	Regulus westerly
		N.B. These were made by the assistant Clock.									
♂	28	Set Mr. Ellicott's clock a going.									
☿	29	32 48 43 29	9 36 25 47 7	39 59 50 44	13 36 26¼	57 20 57 0	29,81	+ 2	0	W.	Arcturus easterly
		29 22 10 6	17 25 43½ 36 29	22 7 32 55		56 0 57 20	29,80	−10	−17	W.	Arcturus westerly
♃	30	5 3 9 37	20 8 52 13 28	12 40 17 16		70 40	29.79	+ 1	+ 8	W.	⊙'s U. L. ⊙'s L. L. easterly
♀	31	6 50 11 25	4 2 59 7 35	59 11 3 40	0 7 45,2	70 40	29,83	+14	+15	W.	⊙'s L. L. ⊙'s U. L. westerly
⊙ April	2	43 24 47 45	19 47 4 51 25	50 42 55 5		72 20	29,85	− 8	− 5	D.	⊙'s U. L. ⊙'s L. L. easterly
☽	3	29 38 34 0	4 25 57 30 20	22 17 26 40	0 8 12,2	72 20	30,00	+ 5	+12	D.	⊙'s L. L. ⊙'s U. L. westerly
♂	4	40 56 45 17	19 44 6 48 58	48 17 52 37		72 0	30,12	− 6	− 3	D.	⊙'s U. L. ⊙'s L. L. easterly
☿	5	32 29	4 28 46 33 15	45 8 29 35		72 0	30,12	+12	+20	D.	⊙'s L. L. ⊙'s U. L. westerly
		38 28 42 50	19 42 7	45 44 50 6		72 0	30,08	− 1	+ 8	D.	⊙'s U. L. ⊙'s L. L. easterly
♃	6	35 16 39 36	4 31 36 35 57	28 0 32 21	0 8 33,4	72 0	30,14	+14	+16	D.	⊙'s L. L. ⊙'s U. L. westerly
♄	8	20 28 24 43	19 24 2 28 17	27 35 31 49½		73 20	30,00	+ 6	+12	W.	⊙'s U. L. ⊙'s L. L. easterly
⊙	9	53 58 58 13	4 50 23 54 39	46 51 51 7½	0 8 50,8	73 20	29,99	+22	+27	W.	⊙'s L. L. ⊙'s U. L. westerly
☽	10	26 16 30 32	19 29 51 34 8	33 24 37 41		72 0	30,20	− 7	− 6	W.	⊙'s U. L. ⊙'s L. L. easterly
♂	11	48 41 52 56½	4 45 5 49 22	41 31 45 49	0 9 7,6	72 0	30,24	+ 8	+12	W.	⊙'s L. L. ⊙'s U. L. westerly
☿	12	13 47:: 18 3	19 17 21 21 35	20 53 25 5		73 0	29,73	+ 5	+12	W.	⊙'s U. L. ⊙'s L. L. easterly
♃	13	1 44 5 57	4 58 10 5 2 24	54 41 58 55	0 9 23,7	73 0	29,63	+23	+17	W.	⊙'s L. L. ⊙'s U. L. westerly

♄ 15 I find from a mean of 8 comparisons made in the course of this week, and that which immediately preceded the last, that the assistant clock gains on Mr. Ellicott's at the rate of 3″, 03 in 6 hours: but from a mean of 4 taken the week preceding the last, it gained only 2″ 81; and from the 4 which were made this week, it gains 3″ 25 in six hours. W. W.

2½″, 3″, 3″, 2″
3″ 4″ 3″ 3″

♄	15	0 29 4 7	20 33 23 38 3	37 16 41 57		62 48	29,91	+ 1	+ 6	D.	⊙'s U. L. ⊙'s L. L. easterly
⊙	16	6 20	3 42 25 47 5	38 32 43 12	0 9 50,−	62 48	29 87	+13	+12	D.	⊙'s L. L. ⊙'s U. L. westerly

1769 April	Equal altitudes. Times by the clock. Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	Zenith distance ° ′	Barometer Inches	Thermometers A	Thermometers B	Observer	Phænomena and circumstances	
♄ 22	14 6	20 17 48	21 32								
	18 33	22 17	26 2		62 40	29,45	+22	+32	W.	☉'s U. L.	easterly
☉ 23	3 48	4 0 1		0 10 46,7						☉'s L. L.	
	8 14	4 4 31			62 40	29,45	36	41	W.	☉'s L. L.	westerly
	3 12	19 6 41								☉'s U. L	
	7 21	10 53	14 22		71 20					☉'s U. L.	easterly
		22 16½	25 46			29,60	26	28	W.	☉'s L. L	flying
	22 59	26 29	29 58		69 20					☉'s U. L.	clouds
☽ 24	59 35	4 56 3:	52 34	0 10 50,8						☉'s L. L.	
	3 49	5 0 17	56 47:		69 20	29,69	35	32	W.	☉'s L L	westerly a little un-
	15 14	11 43	8 13							☉'s U. L.	
	19 26	15 56	12 29		71 20	29,69	34	31	W.	☉'s L. L.	certain because of
	1 0	21	9 15							☉'s U. L.	clouds
	5 54	21 10 6	14 15		56 40	29,72	22	37	W.	☉'s U. L.	easterly
♂ 25	16 48	3 12 37	8 30	0 11 1,5						☉'s L. L.	thin cloud
	21 45	17 36	13 30		56 40	29,75	36	43	W.	☉'s L. L.	westerly
	25 12	19 28 43	32 13							☉'s U. L.	
	29 26	32 57	36 27		68 0	29,67	27	33	W.	☉'s U. L.	easterly
☿ 26	53 39	4 50 8		0 11 7,5						☉'s L. L.	
	57 52	54 23	50 50::		68 0	29,59	43	45	W.	☉'s L. L.	westerly
	33 46:	21 38 21	42 54							☉'s U. L.	very hazy
	39 17	43 54	48 28		52 40	29,66	36	44	W.	☉'s U. L.	easterly
♃ 27	43 50	2 39 11	34 36	0 11 14,3						☉'s L. L.	
	49 22	44 47	40 12		52 40	29.83	49	57	W.	☉'s L. L.	westerly
	21 10	19 24 41	28 10							☉'s U. L.	
	25 23	28 55	32 25		68 0	29,92	33	35	W.	☉'s U. L.	easterly
♀ 28	58 5	4 54 32	51 3	0 11 19,0						☉'s L. L.	
	2 17:	58 45	55 15		68 0	29 99	42	48	W.	☉'s L. L.	westerly
☿ May 3	5 26	20 9 5	12 42							☉'s U. L.	
	9 43	13 20	16 57		61 0	30,20	32	38	D.	☉'s U. L.	easterly
♃ 4	15 43		8 26							☉'s L. L.	
	19 59	4 16 21	12 43	0 12 22½	61 0	30,20	45	49	D.	☉'s L. L.	westerly
	3 34	20 7 10	10 47							☉'s U. L.	
	7 48	11 26	15 2		61 0	30,16	39	47	D.	☉'s U. L.	easterly
♀ 5	17 37	4 13 58	10 22	0 12 22,8						☉'s L. L.	
	21 52	18 14	14 39		61 0	30,03	55	62	D.	☉'s L. L.	westerly
♃ 11	39 32	19 43 6	46 37							☉'s U. L.	
	43 47	47 20	50 51		62 40	29,97	24	22	W.	☉'s U. L.	easterly
♀ 12	44 55	4 41 22	37 53	0 14 3,05						☉'s L. L.	
	49 10	45 37	42 6		62 40	30,12	27	25	W.	☉'s L L.	westerly
	59 56	5 7 36	15 46							☉'s U. L.	
	17 43	27 5	37 45		53 20	30,13	27	25	W.		
	53 49	7 44 29			52 20	30,14	27	24	W.	☽'s U. L. easterly	
	11 29	8 3 55	55 48		52 20	30,15	25	21	W.		
	47 13	8 52 7	57 6		53 20	30,15	24	20	W.	☽'s U. L. westerly	
	2 9	9 7 31	12 59		44 40	30,14	23	17	W.		
	54 52	12 49 31	44 1::	10 58 29,3	43 20	30,14	23	16	W.	Arcturus easterly	
	9 44	13 4 49	59 50		43 20	30,14	19	14	W.		
					44 40	30,14	19	14	W.	Arcturus westerly	

1769 May	Equal altitudes. Times by the clock. Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	Zenith distance ° ′	Barometer Inches	Thermometers A	Thermometers B	Observer	Phænomena and Circumstances.	
☽ 22	49 48:	20 53 36:	57 24		52 20	29,66	32	45	W.	☉'s U. L.	East. unc. because of clouds
	54 21	58 10	1 59							☉'s L. L.	
♂ 23	41 19	3 37 29	33 40	0 17 38,3	52 20	29,75	40	46	W.	☉'s L. L.	west. flying clouds
	5 50	42 3	38 16							☉'s U. L.	
	At 20h put the clock back.								W.		
	10 25	20 14 5	7 45		54 20	29,82	27	39	W.	☉'s U. L.	easterly hazy
	14 48	18 29	22 11	23 56 1,0						☉'s L. L.	
☿ 24	37 48	3 33 56	30 15		54 20	29,89	44	48	W.	☉'s L. L.	westerly hazy
	41 59	38 18	34 39							☉'s U. L.	
♃ 25	14 7	20 17 48	21 31	23 56 43,7	53 40	29,81	34	46	W.	☉'s U. L.	east. very hazy
	18 31	22 13								☉'s L. L.	
♀ 26	5 18	3 31 35			53 40	29,70	47	59	W.	☉'s L. L.	west. flying clouds
	9 42	36 0	32 18							☉'s U. L.	
☉ 27	58 33	19 2 3	5 30½	23 57 50,3	63 0	29,78	31	34	D.	☉'s U. L.	easterly
	2 39	6 12½	9 3½							☉'s L. L.	
	14 1	17 31	20 58		61 0					☉'s U. L.	
	8 8½	21 30	25 6							☉'s L. L.	
☽ 28	37 57:	4 34 23½			61 0	29,85	47	49	D.	☉'s L. L.	west. the ☉ almost covered with clo. at times
	42 3:	33 35.	35 6:							☉'s U. L.	
	53 23	49 50.	46 25		63 0					☉'s L. L.	
	57 28:	53 59:	50 32:							☉'s U. L.	
	57 56	19 1 25	4 52	23 58 12½	63 0	29,88	34	36	D.	☉'s U. L.	easterly
	2 3½	5 31½	8 59							☉'s L. L.	
	13 25	16 54½	20 23		61 0	29,88	35	38	D.	☉'s U. L.	
	7 32½	21 3	24 31							☉'s L. L.	
♂ 30	9 16	4 35 44:			61 0	29,90	49	49	D.	☉'s L. L.	west. very hazy
	43 21	39 51	36 25							☉'s U. L.	
	54 44	51 13½	47 49:		63 0	29,89	49	49	D.	☉'s L. L.	westerly
	58 51	55 21½	51 54½							☉'s U. L.	
♀ June 2	56 5	18 59 34½		23 59 47½	63 0	29,95	35	41	D.	☉'s U. L.	
	0 11	19 3 39½	7 16							☉'s L. L.	
	11 32	15 2	18 31		61 0	29,95	36	42	D.	☉'s U. L.	
	15 40	19 10½	22 37½							☉'s L. L.	
	27 4	30 34½	34 4		59 0	29,95	36	43	D.	☉'s U. L.	easterly
	31 12½	34 43½	38 12½							☉'s L. L.	
	42 40	46 13	49 44		57 0	29,95	37	45	D.	☉'s U. L.	
	46 51½	50 25½	53 56							☉'s L. L.	
♄ 3	12 58	4 9 25			57 0	29,89	52	58	D.	☉'s L. L.	
		13 38	10 8							☉'s U. L.	
	28 41½	25 10½	21 38		59 0	29,89	53	59	D.	☉'s L. L.	
	32 47	29 17½	25 47½							☉'s U. L.	
	44 11	40 41½	37 14		61 0	29,89	53	59	D.	☉'s L. L.	westerly
	48 20	44 50	41 22							☉'s U. L.	
	59 39½	56 11½	52 38½		63 0	29,89	53	60	D.	☉'s L. L.	
	3 49	5 0 18								☉'s U. L.	
		18 59 10	2 36		63 0	29,65	38	42	W.	☉'s U. L.	easterly
	59 51	19 3 20	5 46							☉'s L. L.	

1769

1769		Equal altitudes. Times by the clock.				Zenith distance	Barometer	Thermometers		Phænomena and Circumstances.		
	June	Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	° ′	Inches	A	B	Observer		
♄	3	11 9	14 36	18 2		61 0	29,65	38	42	W.	☉'s U. L.	easterly very hazy
		15 18	18 45	22 12							☉'s L. L.	
		26 37	19 30 7	33 35		59 0	29,65	39	45	W.	☉'s U. L.	
		30 47		37 48							☉'s L. L.	
		42 14	45 47	49 18		57 0	29,64	40	46	W.	☉'s U. L.	
		46 29	50 2	53 32							☉'s L. L.	
☉	4		4 10 34::	7 1:	0 0 9,3	57 0	29,55	52	54	W.	☉'s L. L.	westerly very uncertain because of clou.
				11 18::							☉'s U. L.	
			4 26 14	22 45		59 0	29,55	52	53	W.	☉'s L. L.	
		33 56::	30 28								☉'s U. L.	
		45 17	41 49			61 0	29,55	51	52	W.	☉'s L. L.	
		49 27:	45 56::	42 33							☉'s U. L.	
		18 36	19 22 6			60 0	29,47	37	43	W.	☉'s U. L.	easterly
		22 46:		29 47							☉'s L. L.	
		34 8	37 36	41 7		58 0	29,47	38	43	W.	☉'s U. L.	
		38 18	41 49	45 26							☉'s L. L.	
		49 49	53 22	56 51		56 0	29,47	38	44	W.	☉'s U. L.	
		54 5	59 38	1 9							☉'s L. L.	
☽	5	7 21	4		0 0 34,8	56 0	29,46	49	53	W.	☉'s L. L.	westerly very cloudy and uncertain
			4 8 2								☉'s U. L.	
		23 5		16 2::		58 0	29,46	50	52	W.	☉'s L. L.	
		27 18	23 45::	20 17							☉'s U. L.	
		38 39	35 8	31 40		60 0	29,46	50	52	W.	☉'s L. L.	
		42 51	39 19								☉'s U. L.	
♄	10	10 15½	19 13 44			61 0	29,49	37	41	D.	☉'s U. L.	easterly
		14 23	17 51	21 17½							☉'s L. L.	
		41 16	44 45½	48 17		57 0					☉'s U. L.	
		45 26	48 59	52 30½							☉'s L. L.	
☉	11	21 11	4 17 38	14 8½	0 3 14,3	57 0	29,66	55	54	D.	☉'s L. L.	westerly
		25 22½	21 49½	18 19							☉'s U. L.	
		52 16	48 47	45 20		61 0	29,67	55	55	D.	☉'s L. L.	
		56 25½	52 54½								☉'s U. L.	
☽	19	25 14	19 28 44	32 12		59 20	29,75	44	48	W.	☉'s U. L.	easterly
		29 24	32 55	36 23							☉'s L. L.	
		40 47	44 18	47 47½		57 20					☉'s U. L.	
		44 59	48 30	50 0½							☉'s L. L.	
♂	20	29 56	4 26 25	22 54	0 7 26¼	57 20	29,72	63	63	W.	☉'s L. L.	westerly
		34 7	30 36	27 7							☉'s U. L.	
		45 27	41 57	38 28		59 20					☉'s L. L.	
		49 37	46 7	42 38							☉'s U. L.	
☿	21	26 8½	19 29 38	33 6		59 20	29,74	40	41	W.	☉'s U. L.	easterly
		30 20	33 50	37 19							☉'s L. L.	
		41 43½	45 14	48 43		57 20					☉'s U. L.	
		45 55	49 26	52 55½							☉'s L. L.	
♃	22	30 45	4 27 14	23 43	0 8 19¾	57 20	29,80	60	60	W.	☉'s L. L.	westerly
		34 56	31 26	27 55							☉'s U. L.	
		46 18	42 48	39 18		59 20					☉'s L. L.	
		50 27	46 58	43 30							☉'s U. L.	

1769 June	Equal altitudes. Times by the clock. Lower Wire ′ ″	Middle Wire h ′ ″	Upper Wire ′ ″	Passed the Meridian h ′ ″	Zenith distance ° ′	Barometers Inches	Thermometers A	Thermometers B	Observer	Phænomena and Circumstances.	
♃ 22	11 18	19 14 47			61 20	29,68	46	49	W.	⊙'s U. L.	easterly
	15 27½	18 57	22 24½							⊙'s L. L.	
♀ 23	2 6	4 58 38	55 11	0 8 48,2	61 20	29,53	61	62	W.	⊙'s L. L.	westerly
	6 15	5 2 46	59 19							⊙'s U. L.	
	55 51	19 59 23			55 40	29,51	48	48	W.	⊙'s U. L.	easterly
	0 5	20	7 9:							⊙'s L. L.	
♄ 24	18 18	4 14 45	11 13	0 9 12,8	55 40	29,60	57	56	W.	⊙'s L. L.	westerly
	22 32	18 59	15 27							⊙'s U. L.	
♂ 27	3 49½	20 7 20:	10 56:		55 0	29,69	50	59	D.	⊙'s U. L.	easterly
	8 1	11 34	15 9							⊙'s L. L.	
☿ 28	13 49	4 10 14	6 41½	0 10 58,6	55 0	29,80	66	65	D.	⊙'s L. L.	westerly
	18 0½	14 29	10 56:							⊙'s U. L.	
⊙ July 2	23 59	19 27 28½	30 56		60 40	29,57	52	56	W.	⊙'s U. L.	easterly
	28 8	31 38	35 5							⊙'s L. L.	
☽ 3	57 55	4 54 26	50 58	0 13 8,0	60 40	29,49	70	79	W.	⊙'s L. L.	westerly
	2 4	58 34								⊙'s U. L.	
♃ 6	28 8½	19 31 39	35 8		60 40	30,08	46	50	W.	⊙'s U. L.	easterly
	32 18	35 48½	39 17							⊙'s L. L.	
	43 40	47 10½	50 39½		58 40					⊙'s U. L.	
	47 50½	51 21	54 50:							⊙'s L. L.	
♀ 7	41 29	4 37 58	34 28	0 14 48,1	58 40	30,16	56	54	W.	⊙'s L. L.	westerly
	45 39	42 9	38 39							⊙'s U. L.	
	57 3		50 5		60 40					⊙'s L. L.	
	1 13:	57 44	54 14							⊙'s U. L.	
♃ 13	49 44	19 53 16	56 46		59 0	29,73	48	48	D.	⊙'s U. L.	easterly
	53 54	57 26	0 57							⊙'s L. L.	
♀ 14	40 38	4 37 8	33 37	0 17 27,5	59 0	29,68	55	57	D.	⊙'s L. L.	westerly
		41 17	37 47							⊙'s U. L.	
	56 20½	19 59 53	3 24		58 20	29,60	49	48	D.	⊙'s U. L.	easterly
	0 31	20 4 4	7 35							⊙'s L. L.	
♄ 15	34 40	4 31 8	27 30½	0 17 46,9	58 20	29,65	55	50	D.	⊙'s L. L.	westerly
	38 51	35 19½	31 41							⊙'s U. L.	
♃ 27	55 57½	19 59 29	3 0		61 0	29,61	51	54	D.	⊙'s U. L.	easterly
	0 7	20 3 40½	7 12							⊙'s L. L.	
♀ 28	41 56	4 38 24	34 53	0 21 18,9	61 0	29,67	67	70	D.	⊙'s L. L.	westerly
	46 6½	42 35	39 4							⊙'s U. L.	
♂ Aug. 1	Put the clock back.								W.		
♃ 3	52 4	19 55 40	59 15		61 0	29,65	45	50	W.	⊙'s U. L.	easterly
	56 21½	59 58	3 34							⊙'s L. L.	
♀ 4	14 11	4 10 34½	6 59	0 5 35,4	61 0	29,64	60	56½	W.	⊙'s L. L.	westerly
	18 29	14 53	11 18							⊙'s U. L.	
☽ 7	12 42½	19 16 13	19 42½		67 0	29,50	49	51	D.	⊙'s U. L.	easterly
	16 51½	20 22½								⊙'s L. L.	
♂ 8	54 34	4 51 2½		0 6 5¾	67 0	29,44	56	61	D.	⊙'s L. L.	westerly
	58 44	55 14	51 44							⊙'s U. L.	
♂ 15	34 37½	19 38 12	41 44½		66 20	29,98	46	49	W.	⊙'s U. L.	easterly
	38 55	42 29								⊙'s L. L.	

	Equal altitudes. Times by the clock.				Zenith distance	Barometer	Thermometers		Phænomena and Circumstances.		
	Lower Wire	Middle Wire	Upper Wire	Passed the Meridian		Inches	A	B	Observer		
	′ ″	h ′ ″	′ ″	h ′ ″	° ′						
16	33 37	4 30 2	26 29½	0 6 39,9	66 20	30,01	54	57	W.	☉'s L. L.	westerly
	37 54	34 20	30 47							☉'s U. L.	
24	6 13½	20 9 57½	13 43		65 0	29,54	46	50	D.	☉'s U. L	easterly
	10 39	14 25	16 11							☉'s L. L.	
25	1 39			0 6 34½	65 0	29,64	58	57	D.	☉'s L. L	westerly
	6 7½	4 2 21½								☉'s U. L.	

	Apparent Times.	Zenith distances.				Barometer.	Thermometers.		Phænomena and Circumstances.	
		90 Arch	96 Arch	Subt.	96 Arch reduced	Inches	A	B	Observer	
	h ′ ″	° ′ ″	G. S. V.	″						
5		55 52 33	59 2 12	20	55 51 49	29,61	46	42½	W.	☉'s U. L. on merid.
10		58 20 58	62 0 30	9	58 20 32	30,06	47	41	D.	☉'s L. L. ditto.
11		58 12 30	62 0 11	6	58 12 15	30,09	45½	49	D.	☉'s U. L. ditto.
12		59 7 44	63 0 9	16	59 7 26	29,93	57	60	D.	☉'s L. L. ditto.
7		60 32 24	64 2 10	21	60 32 10	29,77	47	48	W.	☉'s U. L. ditto.
9		50 30 56	53 3 15	6	50 29 56	29,99	52	49	W.	α aquilæ on the merid.
		9 46 57	10 1 24	2	9 47 2	29,97	43	38½	W.	α persei do. pl. qua. E.
	15 40 56	35 19 26	37 2 22	10	35 18 53	29,78	42½	38	W.	☽'s U. L. on merid.
0		61 42 40	65 3 10	25	61 42 25	29,98	47	47½	W.	☉'s U. L. on meri.
		62 15 0	66 1 19	13	62 14 42					☉'s L. L.
1		62 38 15	66 3 9	15	62 38 23	29,79	46	45½	W.	☉'s L. L on merid.
2		14 19 21	15 1 3	16	14 18 52	29,19	46	39	D.	α cygni ditto.
3		50 30 34	53 3 15	4	50 29 58	29,82	32	28½	W.	α aquilæ ditto. (v. g.)
		14 19 25	15 1 3	0	14 19 8	29,82	32	28½	W.	α cygni ditto. (v. g.)
		13 2 44	13 3 22	12::	13 2 54	29,84	28	27	W.	capella do. pl. qu. W.
4		67 2 54	71 2 2	2	67 2 43	29,90	37	34	W.	☉'s U. L. on the merid.
		67 35 25	72 0 12	10	67 35 6					☉'s L. L.
		50 30 49	53 3 16	8	50 30 20	29,90	38	32	W.	α aquilæ ditto.
		9 45 52	10 1 21	0	9 45 47	29,86	31	28	W.	α persei do. pl. qu. W:
		13 3 12	13 3 23	25	13 3 8	29,83	31	27	W.	capell. do. do. ver. hazy
5		68 19 22	72 3 16	17	68 18 56	30,20	25	18½	D.	☉'s L. L. on merid.
		20 12 18	21 2 7	6	20 12 21	30,18	27	21	D.	α lyræ ditto.
		50 30 52	53 3 16	6	50 30 22	30,18	27	21	D.	α aquilæ ditto.
		14 19 30	15 1 4	11	14 19 23	30,17	26	20½	D.	α cygni ditto.
		68 9 20	72 2 25	16	68 8 51	30,22	29	23½	D.	☉'s U. L. ditto.
		20 12 20	21 2 7	2	20 12 25	30,18	29	26½	D.	α lyræ ditto.
		14 19 36	15 1 4	6	14 19 28	30,14	28	23½	D.	α cygni ditto.
		9 46 48	10 1 23	16	9 46 24	30,05	25	21	D.	α persei do. pl. qu. E.
		13 3 34	13 3 23	20	13 3 13	30,01	27	24	D.	capella ditto. ditto.
		50 30 24	53 3 15	14	50 29 48	29,87	19	11½	W.	α aquila on meridian
		14 18 51	15 1 3	18	14 18 50	29,87	17½	10½	W.	α cygni ditto.
		9 45 54	10 1 21	6	9 45 41	30,07	14	9½	W.	α per. do. pl. qu. E. haz.

1768	Apparent Times	Zenith distances. 90 Arch	96 Arch	ub.	96 Arch reduced	Barometer. Inches	Thermometers. A	B	Observer	Phænomena and Circumstances.
November	h ′ ″	° ′ ″	G. S. V.	″	° ′ ″					
☉ 6	.	50 29 11	53 3 12	6	50 28 37	29,65	+ 6	—3	W.	α aquilæ on meridian
		14 18 40	15 1 2	3	14 18 38	29,65	+ 5	—$3\frac{1}{2}$	W.	α cygni ditto.
☿ 9		50 29 38	53 3 14	12	50 29 23	29,89	0	—$3\frac{1}{2}$	W.	α aquilæ ditto.
☿ 16		14 18 48	15 1 2	0	14 18 41	29,58	+ 4	—5	D.	α cygni ditto.

*** Many of the preceding obſervations can be of no uſe in determining the latitude of the place; but I thought it might be uſeful to inſert them, as they ſerve to ſhew what a very great alteration happened in the poſition of the line of collimation of the quadrant, about this time. W. W.

1768	Apparent Times	90 Arch	96 Arch	ub.	96 Arch reduced	Barometer	A	B	Observer	Phænomena and Circumstances
☿ 16		9 45 40	10 1 20	15	9 45 6	29,62	+ 7	— 2	D.	α perſ. on m. pla. qu. E.
		13 2 53	13 3 21	20	13 2 20	29,63	+ 5	— 2	D.	capella ditto ditto.
♀ 18	5 32 43	65 0 0								
	37 30	64 40 0								
	42 31	64 20				29,87	—$6\frac{1}{2}$	—$12\frac{1}{2}$	D.	☽'s L. L. eaſt merid.
	47 50	64 0 0								
	53 11	63 40 0								
	7 28 28	60 11 16	64 2 7	14		29,87	— 8	—13	D.	☽'s L. L. on merid.
	36 28	60 30 20								
	41 28	60 30 50				29,88	— 8	—14	D.	☽'s L. L. weſt merid.
	49 8	60 33 16								
	54 28	60 36 48								
		9 46 32	10 1 23	20	9 46 20	29,88	— 2	—$13\frac{1}{2}$	D.	α perſei } on mer. pl.
		13 3 32	13 3 23	24	13 3 9	29,89	— 1	—14	D.	capella } of qu. W.
		51 25 44	54 3 13	0	51 25 24	29,90	0	—14	D.	α orionis on merid.
♄ 19		9 46 0	10 1 21	0	9 45 47	29,85	—10	—15	D.	α perſei do. pl. qu. E.
☽ 28		9 45 48	10 1 20	4	9 45 17	29,35	— 8	—18	D.	α perſei } on merid. pl.
		13 2 26	13 3 20	4	13 2 10	29,37	—10	—$18\frac{1}{2}$	D.	capella } of quad. E.
♂ 29		9 47 30	10 1 24	3	9 47 3	29,51	— 5	— 9	D.	α perſei } on merid. pl.
		13 3 28	13 3 23	3	13 3 30	29,63	— 4	—12	D.	capella } of quad. W.
♀ Dec. 2		58 11 18	62 0 9	24	58 11 3	29,55	— 5	—16	D.	α urſ. maj. on m. bel. p.
		29 16 20	31 0 28	12	29 15 51	29,53	— 6	—16	D.	polaris do. above pole
		65 2 10	69 1 15	16	65 1 38	29,52	— 6	—16	D.	ζ urſ. maj. do. bel. pole
♄ 3		58 11 30	62 0 9	13	58 11 14	29,56	— 1	— 8	D.	α urſ. maj. do. do.
		29 16 17	31 0 28	12	29 15 51	29,56	— 2	—10	D.	polaris do. above pole
		65 2 7	69 1 15	24	65 1 30	29,56	—$2\frac{1}{2}$	—$10\frac{1}{2}$	D.	ζ urſ. maj. do. bel. pole
		9 46 14	10 1 21	16	9 45 32	29,53	— 3	—12	D.	α perſei do. pl. qu. E.
♂ 6		58 11 32	62 0 9	19	58 11 8	29,94	—$11\frac{1}{2}$	—23	W.	α urſ. maj. do. bel. pole
		9 47 30	10 1 24	0	9 47 7	29,95	9	—23	W.	α perſei do. pl. qu. W.
☿ 7		66 11 48	70 2 13	0	66 11 20	30,10	—10	—$21\frac{1}{2}$	W.	γ urſ. maj. do. bel. pol.
♃ 8		66 11 40	70 2 13	0	66 11 20	30,21	— 3	—$15\frac{1}{2}$	W.	do. do. do.
		65 2 41	69 1 16	4	65 2 17	30,21	— 3	—16	W.	ζ urſ. maj. do. do.
♄ 10		9 46 0	10 1 21	12	9 45 35	29,54	—10	—25	W.	α perſei do. pl. qu. E.
♃ 15		58 11 20	62 0 8	0	58 11 1	29,92	—24	—25	D.	α urſ. maj. do. bel. pol.
		29 15 56	31 0 28	16	29 15 47	29,94	—$23\frac{1}{2}$	—25	D.	polaris do. above pole
		65 1 57	69 0 14	8		29,95	—$23\frac{1}{2}$	—$25\frac{1}{2}$	D.	ζ urſ. maj. do. bel. pole
☽ 19		66 11 39	70 2 13	4	66 11 12	29,89	—15	—21	W.	γ urſ. maj. do. do.
		29 16 16	31 0 28	14	29 15 49	29,89	—15	—21	W.	polaris do. above pole

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1769	Apparent Times	Zenith diſtances. 90 Arch. °	′	″	96 Arch G.	S.	V.	Su. ″	96 Arch reduced °	′	″	Barometer Inches	Thermometers A	B	Obſerver	Phænomena and Circumſtances.
January ☉ 1		70	41	4	75	1	19	11	70	40	59	29,94	—25	—29	W.	η urſ. maj. on mer. bel. the pole
		33	6	28	35	1	8	0	33	6	20	30,16	—25	—27	W.	polaris, ditto, ditto
☽ 2		70	41	9	75	1	19	20	70	40	50	30,19	—28	—34	W.	η urſ. maj. ditto, ditto
♂ 17		42	45	20	45	2	13	0	42	45	5	29,46	—34	—39	W.	aldebaran on the meridian
☿ 18		29	16	16	31	0	28	12	29	15	49	29,45	—34	—38	W.	polaris on merid. above the pole
		65	2	20	69	1	15	20	65	1	34	29,45	—34	—39	W.	ζ urſ. maj. ditto below the pole
		70	41	30	75	1	19	14	70	40	56	29,44	—34	—39	W.	η urſ. maj. ditto, ditto
		42	45	20	45	2	14	20	42	45	12	29,36	—34	—36	W.	aldebaran on the meridian
♀ 20		65	2	27	69	1	15	14	65	1	40	29,45	—27	—30	W.	ζ urſ. maj. on merid. bel. the pole
		2	39	44	2	3	12	12	2	39	46	29,55	—33	—36	W.	ditto, ditto above the pole
		8	20	20	8	3	19	16	8	20	16	29,55	—33	—36	W.	η ditto, ditto, ditto
♄ 21		70	41	28	75	1	19	15	70	40	55	29,74	—31	—36	W.	ditto, ditto below the pole
♄ 28		75	24	10	80	1	22	2	75	23	42	30,13	—37	—40	W.	capella, ditto, ditto
☉ 29		70	41	38	75	1	19	8	70	41	2	30,08	—30	—34	W.	urſ. maj. ditto, ditto
		9	46	7	10	1	21	3	9	45	44	30,07	—30	—35	W.	perſei, ditto above the pole
		13	3	7	13	3	22	0	13	3	6	30,05	—31	—36	W.	capella, ditto, ditto
		2	39	52	2	3	12	14	2	39	44	30,00	—35	—40	W.	urſ. maj. ditto, ditto
		8	19	4	8	3	16	10	8	19	3	30,00	—35	—40	W.	urſ. maj. ditto, ditto
		72	7	52	76	3	24	24	72	7	30	29,99	—36	—41	W.	perſei, ditto below the pole
		75	23	56	80	1	22	16	75	23	28	29,98	—36	—41	W.	capella, ditto
☽ 30		70	41	28	75	1	19	15	70	40	55	29,97	—28	—31	W.	η urſ. maj. ditto, ditto (hazy)
		13	3	8	13	3	22	10	13	2	56	29,96	—29	—30	W.	capella, ditto above the pole
♂ 31		9	40	16	10	1	22	20	9	45	54	30,17	—24	—31	W.	α perſei, ditto, ditto
		13	3	10	13	3	22	12	13	2	54	30,16	—26	—31	W.	capella, ditto, ditto
☉ Feb. 5		13	3	16	13	3	23	20	13	3	13	29,79	—16	—20	D.	capella, ditto, ditto
		51	26	36	54	3	16	24	51	26	19	29,79	—16	—20	D.	α orionis on the meridian
☿ 8		42	45	30	45	2	13	12	42	44	53	30,03	—13	—12	D.	aldebaran, ditto
		13	3	30	13	3	23	10	13	3	23	30,03	—13	—13	D.	capella, ditto above the pole
		51	26	32	54	3	16	24	51	26	19	30,02	—13	—13	D.	α orionis on the meridian
		33	7	16	35	1	10	20	33	6	52	29,83	—13	— 9	D.	polaris, ditto below.
♃ 9		42	45	38	45	2	13	10	42	44	55	29,60	+ 7	+12	D.	aldebaran on the meridian
		13	3	28	13	3	23	24	13	3	19	29,59	+ 9	+12	D.	capella, ditto above the pole
♀ 10		33	7	36	35	1	10	10	33	7	2	30,32	—16	—21	D.	polaris below.
		75	24	17	80	1	23	20	75	23	50	30,33	—21	—26	D.	capella below the pole
☽ 13		75	24	22	80	1	23	18	75	23	52	29,93	—26	—27	W.	ditto, ditto
♂ 14		9	46	26	10	1	22	10	9	46	4	29,98	—17	—21	W.	α perſei on merid. above the pole
		13	3	28	13	3	22	6	13	3	0	29,96	—18	—21	W.	capella, ditto, ditto
☿ 22		42	45	52	45	2	14	0	42	45	32	29,74	—19	—29	D.	aldebaran on the meridian,
		51	26	20	54	3	15	22	51	25	55	29,72	—21	—30	D.	α orionis, ditto
♀ 24		13	3	28	13	3	23	24	13	3	19	29,83	—20	—26	D.	capella, ditto
		51	26	24	54	3	15	20	51	25	57	29,84	—20	—28	D.	α orionis, ditto
		33	7	17	35	1	10	16	33	6	56	29,93	—29	—37	D.	polaris, ditto below the pole
♄ 25		51	26	4	54	3	14	14	51	25	36	29,90	—25	—31	D.	α orionis on the meridian
♃ Mar. 2		42	45	24	45	2	13	3	42	45	2	30,02	—28	—31	W.	aldebaran on the meridian,
♀ 3		42	45	22	45	2	13	0	42	45	5	29,86	—18	—15	W.	ditto, ditto (v. g.)
☽ 6		33	7	20	35	1	10	22	33	6	50	29,70	—31	—36	D.	polaris on the meridian below.
		75	24	34	80	1	24	22	75	24	14	29,69	—36	—41	D.	capella, ditto, ditto.

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1769	Appar. Time	Zenith diftances.				Baro-meter	Thermo-meters			Phænomena and Circumftances.
May	h ′ ″	50 Arch ° ′ ″	96 Arch G. S. V.	Subt. ″	96 Arch reduced ° ′ ″	Inches	A	B	Ob-ferver	
♀ 17	5 57 22	50 57 0								
	6 0 31	54 0								
	4 10	51 0				30,14	+27	+24	W.	☽'s U. L. eaft of the merid.
	6 59	49 0								
	21 5	50 44 52	54 0 15	4	50 44 1½	30,14	+26	24	W.	☽'s U. L. on the merid.
	36 51	49 0								
	39 1	51 0								
	42 58	54 0				30,14	24	22	W.	☽'s U. L. weft of the merid.
	46 12	57 0								
	51 54	51 3 0								
		38 25 32	40 3 29	0	38 24 56	30,14	+21	+15	W.	arcturus on the meridian
☽ 2.		37 59 44	40 2 3	9	37 59 18	29,74	+41	+42	W.	☉'s U. L. ditto
☿ 24		38 9 29	40 2 26	20	38 9 13	29,87	+36	+43	W.	☉'s L. L. ditto (hazy)
☽ June 5		38 25 24	40 3 29	6	38 24 50	29,44	48	44	W.	arcturus on the meridian
☉ 11		64 29 0	68 3 4	18	64 28 39	29,68	54	58	D.	☽'s U. L. ditto
		61 51 50	65 3 29	0	61 51 11					δ } ophiuchi on meridian
		62 53 17	67 0 10	16	62 52 53	29,68	48	44	D.	ε } ophiuchi on meridian
☽ 15	6 9 0	70 20 0				29,78	44	40	D.	
	14 14	10 0							D.	
	19 51	0 0							D.	☽'s U. L. eaft of the merid.
	26 3	69 50 0							D.	
	33 15	69 40 0				29,78	42	40	D.	
	11 10	69 17 5	73 3 19	13	69 16 34	29,79	42	41	D.	☽'s U. L. on the meridian
	46 51	69 40 0							D.	
	54 16	69 50 0				29,79	42	41	D.	☽'s U. L. weft of the merid.
	8 0 44	70 0 0							D.	
♂ 20		35 3 31	37 1 19	22	35 3 18	29,76	57	64	W.	☉'s U. L. } on the merid.
		35 35 6	37 3 26	12	35 54 40					☉'s L. L. }
♃ 22		35 56 12	37 3 28	6	35 55 39	29,79	54	56	W.	☉'s L. L. } on the merid.
		35 4 20	37 1 20	16	35 3 50					☉'s U. L. }
♄ 24		6 19 22	6 2 31	20	6 18 53	29,57	52	49	W.	β } draconis on the meridian,
		7 16 20	7 3 1	18	7 16 5	29,57	51	48	W.	γ } plane of the quadrant eaft.
☉ July 2		7 15 36	7 3 0	13	7 15 43	29,68	56	52	W.	γ drac. do. pl. of quadr. weft
♀ 7		6 18 56	6 2 30	11::	6 18 38	30,14	50	44	W.	β } draconis ditto, plane of the
		7 16 22	7 3 1	4	7 16 19	30,13	49	43	W.	γ } quadrant eaft (foggy)
♂ 18		7 15 43	7 3 0	14	7 15 42	29,44	57	53	W.	γ drac. do. pl. of quadr. weft
♃ 20		6 18 46	6 2 30	18	6 18 31	29,64	54	50	W.	β } draconis, plane of the
		7 15 36	7 3 0	18	7 15 38	29,64	53	48	W.	γ } quadrant weft
♀ 21		6 19 27	6 3 0	25	6 19 16	29,66	56	52	W.	β } draconis on the meridian,
		7 16 18	7 3 1	10	7 16 13	29,66	56	51	W.	γ } plane of the quad. eaft
♄ 22		6 18 30	6 2 29	2	6 18 24	29,63	54	50	W.	β } draconis on the meridian,
		7 15 40	7 3 0	12	7 15 44	29,63	54	49	W.	γ } plane of the quad. weft
♄ 29		7 16 23	7 3 1	8	7 16 15	29,69	62	58	D.	γ drac. do. pl. of quadr. eaft
☉ 30		7 15 30	7 3 0	20	7 15 36	29,67			W.	γ drac. on mer. pl. of quad. weft
☿ Aug. 2		40 55 30	43 2 19	8	40 55 5½	29,73	57	59	W.	☉'s U. L. } on merid. clouds
		41 27 28:	44 0 28	10	41 27 8					☉'s L. L. }
♀ 4		7 16 20	7 3 1	16	7 16 7	29,62	55	47	W.	γ drac. do. pl. of quadr. eaft

1769	Apparent Times	Zenith distances. 90 Arch	96 Arch	Su.	96 Arch reduced	Barometer	Thermometers A	Thermometers B	Observer	Phænomena and Circumstances.
August	h ′ ″	° ′ ″	G. S. V.	″	° ′ ″	Inches	A	B	Observer	
♄ 5		42 15 36	45 0 9	0	42 15 12	29,67	55	54	W.	☉'s L. L. on the merid.
		41 43 46	44 2 2	23	41 43 37				W.	☉'s U. L. on the merid.
	2 37 1	65 14 21	69 2 11	21	65 13 52					
	42 6	65 11 53	69 2 6	25	11 36					
	46 29	65 10 50	2 3	18	10 24					
	54 17	65 10 0	2 1	10	9 39	29,70	59	57	W.	☽'s U. L.
	58 59	65 10 34	2 2	18	9 57					
	3 4 [illegible]	65 12 20	2 6	6	11 55					
	9 17	65 14 22	2 10	0	65 13 46					
		65 21 4:	69 2 25:	0	65 20 22	29,79	51	47	W.	β aquarii on the meridian
		60 12 36	64 0 28	18	60 12 0	29,80	51	47	W.	α aquarii on the meridian
♄ 12		44 14 50	47 0 25	15	44 14 29	29,63	56	61	D.	☉'s L. L. on the merid.
☉ 20	14 20 28	48 40 0								
	23 14	48 35 0								
	28 43	48 31 0				29,81	44	44	D.	☽'s L. L. east of merid.
	20 53	48 27 0								
	31 48::	48 23 0								
		48 5 42	51 1 6	18	48 5 9	29,81	45	44	D.	Ditto on the meridian
	15 29 2[illegible]	48 23 0								
	32 26	48 27 0								
	35 19	48 31 0				29,81	45	43	D.	Ditto west of the merid.
	37 53	48 35 0								
	40 5[illegible]	48 40 0								
♂ 22		47 30 19	50 2 20	0	47 29 25	29,48	54	59		☉'s L. L. on the merid.

1768	Time per clock	Apparent Time	Occultations of Fixed Stars by the Moon, &c. Observed.
September	h ′ ″	h ′ ″	
☿ 21	7 2 9	7 6 52	ρ ♑ immerged behind the ☽'s dark limb J. D.
	7 2 16	7 6 59	W. W.
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☿ Mar. 15	11 21 6	11 24 34	ζ ♊ immerged behind the ☽'s dark limb (very exact) W. W.
	12 8 44	12 12 11	Ditto emerged (perhaps about 5″ sooner) J. D.
☿ 29	16 54 0	16 46 22::	2d ν ♐ immerged behind the bright limb of the moon W. W.
	53 58	16 46 19	J. D.
☉ Apr. 9	10 29 21	10 20 27½	τ ♉ immerged behind the Moon's dark limb J. D.
☽ 10	15 38 44	15 29 39	♃'s 1st satellite immerged close to the body of the planet W. W.
	39 14	15 30 9	J. D.
☉ Aug. 11	9 16 47	9 10 22½	The ✻ N° 43 of Ophiuchi in Mr. Flamstead's catalogue immerged behind the dark limb of the ☽ (very faint) J. D.
	10 14 56	10 8 31	B in the same constellation and catalogue immerged J. D.
	10 14 54	10 8 29	Ditto per W. W. N. B. The immersion happened towards the northern limb of the ☽ so very near the intersection of light and darkness, as to render the observation doubtful to 2 or 3″.

The

The following Table for the MICROMETER I received from the late Mr. Short, along with the Instrument.

Wm. Wales.

Inches	′ ″	Decim. of an in.	′ ″	Vern^r	″	Vern^r	″
1	6 50,2	0,05	0 20,5	1	0,8	20	16,4
2	13 40,4	10	0 41,0	2	1,6	21	17,2
3	20 30,6	15	1 1,5	3	2,5	22	18,1
4	27 20,9	20	1 22,0	4	3,3	23	18,9
5	34 11,1	25	1 42,6	5	4,1	24	19 7
		30	2 3,1	6	4,9		
		35	2 23,6	7	5,7		
		40	2 44,1	8	6,6		
		45	3 4,6	9	7,4		
		50	3 25,1	10	8,2		
		55	3 45,6	11	9,0		
		60	4 6,1	12	9,8		
		65	4 26,6	13	10,7		
		70	4 47,2	14	11,5		
		75	5 7,7	15	12,3		
		80	5 28,2	16	13,1		
		85	5 48,7	17	13,9		
		90	6 9,2	18	14,8		
		95	6 29,7	19	15,6		

1769	Times per clock			Apparent Times			Parts of the micrometer		Micrometer reduced		Observations on the Transit of Venus.	
June	h	′	″	h	′	″	Inches	Vernier	′	″		
♄ 3	0	56	49	0	57	0,6					Exterior contact at the ingress	J. D.
		56	56	0	57	7,6					Ditto	W. W.
	1	15	10	1	15	21,3					Interior ditto	W. W.
		15	14	1	15	25,3					Ditto	J. D.
		57	21		57	31½	0,40	18	2	57,5	Dist. of ♀'s farthest limb from the ⊙'s nearest	
		58	36		58	46½	0,10	22	0	57,5	♀'s diameter off the scale	
	2	1	16	2	1	26½	4,60	4½	31	32,3	⊙'s diameter	
		2									Cloudy a short time	
		4	11		4	21½	4,25	10	29	13,3	Dist. of Venus's farthest limb from the ⊙'s farthest	J. D.
		5	58		6	8½	0,10	19	0	58,2	♀'s diameter on the scale	
		7	33		7	43½	0,50	0½	3	27,1	Dist. of ♀'s farthest limb from the ⊙'s nearest	
		9	9		9	19½	0,10	19	0	58,2	♀'s diameter on the scale	
		10	26		10	36¼	0,15	0	0	59,9	Ditto off the scale	
		12	0								Cloudy	

1769	Times per clock	Apparent times	Parts of the micrometer: Inches	Parts of the micrometer: Vernier	Micrometer reduced	Obſervations on the Tranſit of Venus.	
June	h ′ ″	h ′ ″	Inches	Vernier	′ ″		
♄ 3	2 39 0	2 39 10	4,60	1½	31 29,8	☉'s horizontal diameter	
			4,60	1	31 29,4		
	44 43	44 52¾	0,70	7½	4 54,9	Diſt. of ♀'s fartheſt limb from the ☉'s neareſt	
	51 40	51 49¾	0,10	20½	0 59,4	♀'s diameter on the ſcale of the micrometer	
	53 26	53 35½	0,15	2	1 1,5	Ditto off	
	3 4 58	3 5 7½	0,80	4	5 33,1	Diſt. of ♀'s fartheſt limb from the ☉'s neareſt	
	6 13	6 22½	0,10	21	0 59,8	♀'s diameter on the ſcale	
						Cloudy	W. W.
	17 47	17 56¼	4,60	4	31 31,9	☉'s inclined diameter	
	19 40	19 49¼	0,85	6:	5 55,2	Diſt. of ♀'s fartheſt limb from the ☉'s neareſt	
	22 20	22 29¼	0,85	14:	6 1,8	Ditto	
	23 38	23 47	0,10	21	0 59,8	♀'s diameter on the ſcale	
	24 35	24 44	0,10	23½	0 58,7	Ditto off	
	42 47	42 55¼	0,10	24	0 59,1	Ditto ditto	
						N. B. Several of the above obſervations are a little uncertain, being taken in great haſte, in the intervals between flying clouds. W. W.	
	46 40	46 48¾	0,90	10½	6 19,4	Diſt. of ♀'s fartheſt limb from the ☉'s neareſt	
	48 0					A ſmall cloud	
	48 49	48 57¾	0,90	11	6 19,8	Diſt. of ♀'s fartheſt limb from the Sun's neareſt	
	51 33	51 41¾	0,90	12	6 20,6	Ditto	J. D.
	55 24	55 32½	0,90	12	6 20,6	Ditto	
	56 19	56 27½	0,90	12½	6 21,1	Ditto	
	59 2	59 10½	0,90	12:	6 20,6	Ditto	
	4 0 50	4 0 58½	0,90	11½	6 20,2	Ditto	
	2 51	2 59½	0,90	11½	6 20,2	Ditto	
	5 23	5 31½	0,90	12½	6 21,1	Ditto	
	7 12	7 20½	0,90	12½	6 21,1	Ditto	
	11 5	11 13¼	0,90	12	6 20,7	Ditto	
	14 37	14 45¼	0,90	12	6 20,7	Ditto	
	17 50	17 58¼	0,90	11	6 19,8	Ditto	
	19 50	19 58¼	0,10	22	1 0,7	♀'s diameter on the ſcale	
	21 30	21 38¼	0,10	24	0 59,1	Ditto off	
	23 27	23 35	4,60	0½	31 29,0		
	25 42	25 50	4,60	1½	31 29,8	☉'s inclined diameters	W. W.
	27 12	27 20	4,60	0½	31 29,0		
	28 42	28 50	4,60	2	31 30,2		
	30 56	31 4	0,90	8		Diſt. of ♀'s fartheſt limb from the Sun's neareſt	
	35 39	35 47	0,90	5		Ditto	
	44 25	44 32¾	0,85	19		Ditto	
	46 14	46 21¾	0,84	17		Ditto	
	50 16	50 23¾	0,85	8		Ditto	
	57 20	57 27½	0,80	20		Ditto	
	5 32 55	5 32 2	0,70	3	4 46,4	Ditto	
	34 52	34 59	0,65	23½	4 42,6	Ditto	
	41¾	41 51¾	0,15	2	0 59,8	♀'s diameter on the ſcale	J. D.
	42½	42 36¼	0,10	17	0 58,2	Ditto off	

1769	Times per clock	Apparent times	Parts of the micrometer: Inches	Parts of the micrometer: Vernier	Micrometer reduced	Observations on the Transit of Venus continued.	
June	h ′ ″	h ′ ″			′ ″		
♄ 3	5 43 37	5 43 43¾	0,15	2	0 59,8	♀'s diameter on the scale	J. D.
	45¾	45 51¾	0,10	21	1 1,5	Ditto off	J. D.
			0,15	2	0 59,8	Ditto on	J. D.
	53—	53 6½	4,60	13½	31 34,8	☉'s horizontal diameter	J. D.
	55—	55 6½	4,60	14	31 35,2	☉'s horizontal diameter	J. D.
	59¼	59 21½	0,15	4	1 1,5	♀'s diameter on the scale	J. D.
	6 0 41	6 0 47½	0,10	18	0 59,1	Ditto off	J. D.
	1 49	1 55½	0,10	21	1 1,5	Ditto ditto	J. D.
	3 30	3 30½	0,50	18	3 36,6	Dist. of ♀'s farthest limb from the ☉'s nearest	J. D.
	4 40	4 46½	0,50	14½	3 33,7	Ditto	J. D.
	6 55	7 1¼	0,50	11	3 30,8	Ditto	J. D.
	8 15	8 21¼	0,50	9	3 29,2	Ditto	J. D.
	15 6	15 12¼	0,45	15	3 13,6	Ditto	W. W.
	17 6	17 12½	0,45	8	3 7,9	Ditto	W. W.
	19 6	19 12¼	0,45	1	3 2,1	Ditto	W. W.
	21 5	21 11¼	0,40	21	2 58,0	Ditto	W. W.
	25 27	25 33	0,40	6	2 45,7	Ditto	W. W.
	20 59	27 1	0,40	2 :	2 42,4	Ditto } hazy	W. W.
	28 19	28 25	0,40	0 : :	2 40,8	Ditto } hazy	W. W.
	7 0 40	7 0 45½	The thread of light broke at the internal contact				W. W.
	0 43	7 0 48½	Ditto				J. D.
	18 56	7 19 1¼	The external contact } very hazy, and the limbs badly defined				W. W.
	19 15	7 19 20¼	Ditto } very hazy, and the limbs badly defined				J. D.

R E M A R K S.

1. All the measurements of Venus's diameter; and also all those of the Sun, which are not said to be horizontal, were taken with the micrometer, in the same direction that the last preceding distance of the limbs of Venus and the Sun was measured with.

2. We were obliged to alter the rack-work of the micrometer before we began to measure any distances of the limbs, &c. in order to make it take in the diameter of Venus, off the scale.

3. The heavens at the beginning, and for a considerable time both before and after, were frequently obscured by clouds: but in the intervals, the air was very clear, and the Sun's limbs extreamely well defined.

4. Soon after Venus was half immerged, a bright crescent, or rim of light, encompassed all that part of her circumference which was off the Sun; thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually.

5. We took for the instant of the first internal contact, the time when the least visible thread of light appeared behind the subsequent limb of Venus: but before that time, Venus's limb seemed within that of the Sun, and his limb appeared behind hers in two very obtuse points, seeming as if they would run together in a broad stream, like two drops of oil; but which nevertheless did not happen, but joined in a very fine thread, at some distance fromthe exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress; owing, as we apprehend, to the bad state of the air at that time. We took for the instant of internal contact, at the egress, the time when the thread of light disappeared before the preceeding limb of the planet, from which time W. W. took notice that he had told about 24″ when the limbs of the Sun and Venus were apparently in contact; a circumstance which he did not venture to attend to at the ingress.

6. We saw nothing like the appearance of an atmosphere round Venus (unless the above-mentioned phænomena may be thought to proceed from thence) either at the beginning, end, or during the time of the transit: nor could we see any thing of a satellite; though we looked for it several times.

7. It may not be improper to add, that the haziness, complained of at the egress, was not owing to any accidental bad quality of the air at that time; it is continually so here to 10° or 12° above the horizon, and often even to 16° or 18°, in what may be called the clearest state of the heavens.

Observations

Obſervations for determining the Magnetic Variations at Prince of Wales's Fort on the North-weſt Coaſt of Hudſon's Bay, by W. W.

The variation compaſs, which I received from Mr. Robertſon, by order of the Royal Society, was, when I received it, a very good one, as appeared to me from ſeveral trials which I made of it in London, before it was put on board the ſhip; but when we arrived in Hudſon's Bay, and were ready to make uſe of it, we had the mortification to find that the needle thereof had, by ſome cauſe or other, entirely loſt its magnetic virtue. As the cold was, by the time that we made this diſcovery, much more intenſe than it probably was at the time that Mr. Ellis complains of a ſimilar circumſtance happening to him in thoſe parts, I was naturally led to try whether I could not benefit by his experience, and accordingly removed the compaſs into the room where we lived; which was kept very warm by a large fire, and by the houſe ſtove; and there it remained ever after, but without the leaſt effect.

In order to remedy this misfortune as much as lay in my power, I applied to Captain Richards, as ſoon as he arrived in the river this year; and deſired he would ſend me his azimuth compaſs on ſhore, with which requeſt he very kindly complied the next day; but the cloudy weather prevented me from making any obſervations before the 22d of Auguſt.

The compaſs is of the common form, and I judged that it would be beſt to make the obſervations about noon, when the Sun's azimuths change the faſteſt, and to note the times by the clock; which I did in the following manner:

1769	Times by the clock	Magnetic azimuth	Variation weſt	
Auguſt	h ′ ″	° ′	° ′	
☽ 21	23 40 29	1 23 W	10 6	Theſe obſervations were made by Captain Richards; the compaſs having been removed, and the card re-adjuſted after the firſt
	44 48	2 0 W	9 17	
	49 35	3 20 W	9 2	
	52 53	4 30 W	9 6	
♂ 22	0 6 40,1			The Sun tranſited the meridian
	23 18	15 15 W	9 43	By myſelf; the compaſs having been moved, and the card re-adjuſted
	25 22	16 17 W	10 3	
	27 8	16 39 W	9 50	
	23 28 59	2 38 E	9 49	The compaſs placed as it was yeſterday before noon
	31 15	1 50 E	9 52	
	34 5	0 47 E	9 59½	
	42 20	2 0 W	10 3½	The compaſs removed, &c.
	44 10	2 38 W	10 4½	
	45 40	3 12 W	10 7½	
	49 45	4 21 W	9 57	Ditto
	52 44	5 20 W	9 51	
	54 4	5 40 W	9 50	
☿ 23	0 6 38,6			The Sun tranſited the meridian
	12 43	11 25 W	9 24	The compaſs removed, &c.
	25 13	15 28 W	9 18	The compaſs again removed, and the card re-adjuſted
	26 26	16 7 W	9 33	
	28 8	16 30 W	9 22	
	34 38	18 40 W	9 24	Ditto
	36 16	19 22 W	9 33½	
The mean is			9 41½	

Such

Such are the beſt obſervations of this kind, which I am able to lay before this honourable and learned Society. It gives me much concern to find that they differ ſo widely from one another; more eſpecially as I am certain that I made them with all the care and circumſpection that I was capable of, and with an inſtrument which ſeemed to me good of its kind. But I flatter myſelf it will be conſidered, that, in making obſervations with this inſtrument, there are two unavoidable ſources of error, viz. in adjuſting the card to the line on the ſide of the compaſs-box, and in making the ſhadow of the thread to fall on the line of the index: I may likewiſe add a third error, which may be committed in reading of the vernier, as it only ſubdivides to every 5′; and if all theſe ſhould happen to fall the ſame way, their ſum, I preſume, may be conſiderable (when an inſtrument of ſo ſmall a radius is uſed), in the hands of the moſt ſkilful obſerver.

The Latitude of Prince of Wales's Fort on the North-weſt coaſt of Hudſon's Bay, deduced from Obſervations of circumpolar Stars.

1769 By ζ Urſæ Majoris

Date of the Obſervation. Above the pole		Below the pole		Latitude deduced 90 Arch °	′	″	96 Arch °	′	″
Jan.	20	Jan.	18	58	47	29½	58	47	53
			20	58	47	27	58	47	51
	29		18	58	47	33½	58	47	52
			20	58	47	29	58	47	50
The means of theſe are				58	47	30¼	58	47	51½

1769 By η Urſæ Majoris

Above the pole		Below the pole		90 Arch °	′	″	96 Arch °	′	″
Jan.	20	Jan.	18	58	47	51	58	48	6
			21	58	47	42	58	48	7
			29	58	47	46¾	58	48	2½
			30	58	47	53	58	48	7½
	29		18	58	47	14	58	47	30½
			21	58	47	14¾	58	47	30¾
			29	58	47	9¼	58	47	36¼
			30	58	47	15½	58	47	32½
Means of theſe are				58	47	32	58	47	48

1769 By Capella

Date of the Obſervation. Above the pole		Below the pole		Latitude deduced 90 Arch °	′	″	96 Arch °	′	″
Jan.	29	Jan.	28	58	47	23	58	47	36¾
	30			58	47	23½	58	47	31½
	31			58	47	24½	58	47	30¾
Febr.	5			58	47	28¼	58	47	39¼
	8			58	47	34¼	58	47	48½
	9			58	47	31¼	58	47	37
Jan.	29	Jan.	29	58	47	30½	58	47	44½
	30			58	47	30½	58	47	38¾
	31			58	47	31¾	58	47	38
Febr.	5			58	47	34¼	58	47	46¾
	8			58	47	41¾	58	47	51¾
	9			58	47	40¼	58	47	44¼
Jan.	29	Feb.	10	58	47	22½	58	47	35¼
	30			58	47	23	58	47	30½
	31			58	47	24	58	47	29¾
Febr.	5			58	47	26¾	58	47	38½
	8			58	47	33¾	58	47	43½
	9			58	47	32¼	58	47	36
	14			58	47	32¾	58	47	32½
Jan.	29	Febr.	13	58	47	21	58	47	35¼
	30			58	47	21½	58	47	30½

1769 By Capella continued

Date of the Observation		Latitude deduced	
Above the pole	Below the pole	90 Arch	96 Arch
		° ′ ″	° ′ ″
Jan. 31	Febr. 13	58 47 22½	58 47 29¾
Febr. 5		58 47 25¼	58 47 38½
8		58 47 32¼	58 47 43½
9		58 47 30¾	58 47 36
14		58 47 31¼	58 47 32½
24		58 47 31¼	58 47 36¼
Means of these are		58 47 29	58 47 37½

By α Persei

Jan. 29	Jan. 29	58 47 23¾	58 47 28½
31		58 47 28¼	58 47 33¼
Febr. 14		58 47 33	58 47 38
Means of these are		58 47 28⅓	58 47 33¼

*** These four stars passed the meridian to the southward of the zenith, when above the pole; which circumstance rendered them vastly convenient for determining the latitude of the place, as the error of the line of collimation of the quadrant is thereby entirely excluded, provided it did not alter in the interval between the observations.

1768 By the Pole Star

Date of the Observation		Latitude deduced	
Above the pole	Below the pole	90 Arch	96 Arch
	1769	° ′ ″	° ′ ″
Decem. 2	January 1	58 47 25½	58 47 37
	Febr. 8	58 47 6	58 47 24½
	24	58 47 6½	58 47 24
3	January 1	58 47 32½	58 47 33
	Febr. 8	58 47 7½	58 47 24½
	24	58 47 8½	58 47 24
15	January 1	58 47 37	58 47 38½
	Febr. 8	58 47 18½	58 47 26
	24	58 47 18	58 47 25½
19	January 1	58 47 25½	58 47 37½
	Febr. 8	58 47 7½	58 47 25
1769	24	58 47 8½	58 47 24½
Jan. 18	January 1	58 47 25½	58 47 35½
	Febr. 8	58 47 6	58 47 23
	24	58 47 7½	58 47 22½
Means of these are		58 47 16	58 47 28½

The Latitude of Prince of Wales's Fort deduced from Observations of the Sun, and of such stars as passed south of the zenith.

By the Sun		Latitude deduced	
1768	Declination	90 Arch	96 Arch
	° ′ ″ N.	° ′ ″	° ′ ″
Sept. 20	0 42 9	58 48 19	58 47 57
21	0 18 44½	58 48 26½	58 48 14½
	S.		
22	0 4 40	58 48 15½	58 48 2
30	3 11 59½	58 48 15½	58 48 3
Oct. 1	3 35 19½	58 48 21½	58 48 33
14	8 32 49	58 48 18½	58 48 12
17	9 39 4	58 48 32½	58 48 7½
1769	N.		
June 20	23 28 5½	58 47 38½	58 47 26
22	23 27 40	58 48 10½	58 47 46
August 2	17 36 18½	58 48 12½	58 48 6½
5	16 48 8½	58 48 16	58 48 7
22	11 33 11	58 48 14	58 47 27½
The means of these are		58 48 15	58 48 0

By Capella		Latitude deduced	
1768	Declination	90 Arch	96 Arch
	° ′ ″	° ′ ″	° ′ ″
Oct. 13	45 44 10,3	58 47 34½	58 47 33½
14		58 48 3½	58 47 40½
17		58 47 48	58 47 39½
The means of these are		58 47 48½	58 47 39½

By α Persei

Sept. 29	49 1 9	58 47 47	58 47 53½
Oct. 14	49 1 10	58 47 47	58 47 39½
17		58 47 44	58 47 21½
The means of these are		58 47 46	58 47 38¾

By α Lyræ 1768	Declination ° ′ ″	Latitude deduced: 90 Arch ° ′ ″	Latitude deduced: 96 Arch ° ′ ″
Oct. 16	38 34 49	58 47 21½	58 47 31½
17		58 47 24	48 47 35½
The means of these are		58 47 22¾	58 47 33½

By α Aquilæ

Sept. 29	8 16 22½	58 47 51½	58 47 18½
Oct. 13		58 47 52	58 47 23
14		58 48 6	58 47 44
16		58 48 11	58 47 48½
The means of these are		58 48 0	58 47 33½

By α Cygni

Oct. 2	44 27 48	58 47 21	58 47 0
13		58 47 26½	58 47 17
16		58 47 32	58 47 32
17		58 47 38	58 47 37
The means of these are		58 47 29½	58 47 21½

By α Persei

Nov. 16	49 1 11	58 47 50	58 47 20½
18		58 47 29	58 47 12½
19		58 48 11	58 48 2
28		58 48 0	58 47 34
Decem. 10		58 48 14	58 48 2
The means of these are		58 47 57	58 47 38

By Capella 1768	Declination ° ′ ″	Latitude deduced: 90 Arch ° ′ ″	Latitude deduced: 96 Arch ° ′ ″
Nov. 16	45 44 10½	58 47 54,2	58 47 29,0
18		58 47 43,–	58 47 12,2
28		58 47 26,7	58 47 18,5
29		58 47 38,9	58 47 32,1
The means of these are		58 47 40,7	58 47 23,–

1769 By β Draconis

June 24	52 28 50	58 47 59	58 47 36
July 20		58 48 14	58 47 55
21		58 48 13	58 48 5
22		58 47 58	58 47 45
The means of these are		58 48 6	58 47 50¼

By γ Draconis

June 24	51 31 29½	58 47 37	58 47 29
July 2		58 47 38½	58 47 40
7		58 47 43	38 47 46½
18		58 47 50½	58 47 43
20		58 47 44	58 47 40
21		58 47 43	58 47 44
22		58 47 48½	58 47 46
28		58 47 50	58 47 48
30		58 47 40½	58 47 39
August 4		58 47 48½	58 47 41
The means of these are		58 47 44½	58 47 41½

The Latitude of Prince of Wales's Fort deduced from Observations of Stars on the Northern Meridian.

By α Ursæ Majoris 1768	Polar dist. ° ′ ″	Latitude deduced: 90 Arch ° ′ ″	Latitude deduced: 96 Arch ° ′ ″
Decem. 2	27 0 24	58 47 14	58 47 21
3		58 47 3	58 47 12
6		58 46 57	58 47 14
15		58 47 8	58 47 20
The means of these are		58 47 5½	58 47 16¾

By γ Ursæ Majoris 1768	Polar dist. ° ′ ″	Latitude deduced: 90 Arch ° ′ ″	Latitude deduced: 96 Arch ° ′ ″
Decem. 15	35 1 15	58 46 50	58 47 10
		58 46 58	58 47 8
		58 46 59	58 47 19
The means of these are		58 46 55⅔	58 47 12⅓

By η Ursæ Majoris		Latitude deduced	
1769	Polar dist.	90 Arch	96 Arch
	° ′ ″	° ′ ″	° ′ ″
January 1	59 31 35	58 47 10	58 47 8
2		58 47 2	58 47 14
The means of these are		58 47 6	58 47 11

	90 Arch ° ′ ″	96 Arch ° ′ ″
The means of all the comparisons of ζ Ursæ Majoris	58 47 $30\frac{1}{4}$	58 47 $51\frac{1}{2}$
Ditto of η Ursæ Majoris (considered as circumpolar)	58 47 32	58 47 48
Ditto of Capella ditto	58 47 29	58 47 $37\frac{1}{2}$
Ditto of α Persei ditto	58 47 $28\frac{1}{2}$	58 47 $33\frac{1}{4}$
Ditto of the Pole star	58 47 16	58 47 28
The means of all the circumpolar stars are	58 47 27	58 47 $39\frac{1}{4}$
The means of α Ursæ Majoris	58 47 $5\frac{1}{2}$	58 47 $16\frac{3}{4}$
Ditto of γ	58 46 $55\frac{1}{3}$	58 47 $12\frac{1}{3}$
Ditto of η	58 47 6	58 47 11
The means of all the stars taken on the northern meridian are	58 47 2,4	58 47 $13\frac{1}{3}$
And the means of the above two are	58 47 $14\frac{3}{4}$	58 47 $26\frac{1}{2}$
The means of all the solar observations are	58 48 15	58 48 0
Ditto of Capella taken on the southern meridian alone	58 47 $48\frac{1}{2}$	58 47 $39\frac{1}{2}$
Ditto of α Persei	58 47 46	58 47 $38\frac{1}{4}$
Ditto of α Lyræ	58 47 $22\frac{3}{4}$	58 47 $33\frac{1}{2}$
Ditto of α Aquilæ	58 48 0	58 47 $33\frac{1}{2}$
Ditto of α Cygni	58 47 $29\frac{1}{2}$	58 47 $21\frac{1}{2}$
Ditto of Capella after the line of Collimation altered	58 47 40,7	58 47 23
Ditto of α Persei ditto	58 47 57	58 47 38
Ditto of β Draconis, the line of collimation having again altered	58 48 6	58 47 $50\frac{1}{4}$
Ditto of γ ———	58 47 $44\frac{1}{2}$	58 47 $41\frac{1}{2}$
The means of all the observations taken southward of the zenith are	58 47 49	58 47 38
The means of the circumpolar and northern stars	58 47 $14\frac{3}{4}$	58 47 $26\frac{1}{2}$
And, by taking the mean of both, the latitude is North	58 47 32	58 47 $32\frac{1}{4}$

The error of the line of collimation of the quadrant was 23″,6 for the 90 arch, and 19″,7 for the 96 arch, to be subtracted from all zenith distances, from the beginning of September, 1768, to the latter end of October; from about which time, till towards the latter end of December, it appears to have been 29″,4′ for the 90° arch, and 36″,7 for the 96 arch, to be added to all zenith distances taken in that interval. About the latter end of December it altered again, but I had no opportunity of determining its quantity, and seemed to be pretty constant all the month of January, 1769; but, about the beginning or middle of February, it began again to alter, and continued uncertain until the middle or latter end of June, when it became constant again, and seemed to me to be, by the observations of β and γ Draconis, 21″,6 and 15″,5 to be subtracted from the 90 and 96 arches, respectively.

W. W.

A TABLE of the EQUATIONS to Equal Altitudes. Lat. 58° 47′½.

Half the Interval between the Obfervations.

The ☉'s longitude	h ′ II 20	h ′ II 30	h ′ II 40	h ′ II 50	h ′ III 0	h ′ III 10	h ′ III 20	h ′ III 30	h ′ III 40	h ′ III 50	h ′ IV 0	h ′ IV 10	h ′ IV 20	h ′ IV 30	h ′ IV 40	h ′ IV 50	h ′ V 0	h ′ V 10	h ′ V 20	h ′ V 30
0 — 0	26,6	26,8	27,0	27,3	27,6	27,9	28,3	28,7	29,2	29,6	30,1	30,6	31,2	31,8	32,4	33,1	33,8	34,5	[illegible]	36,4
5	25,9	26,1	26,4	26,7	27,0	27,4	27,8	28,2	28,7	29,1	29,6	30,1	30,7	31,3	31,9	32,6	33,3	34,1	35,0	35,9
10	25,2	25,4	25,7	26,0	26,3	26,7	27,1	27,5	28,0	28,4	28,9	29,5	30,1	30,7	31,3	32,0	32,7	33,5	34,4	35,3
15	24,3	24,5	24,8	25,1	25,4	25,8	26,2	26,6	27,1	27,6	28,1	28,6	29,2	29,8	30,5	31,2	31,9	32,6	33,6	34,5
20	23,2	23,5	23,8	24,1	24,4	24,8	25,2	25,6	26,1	26,6	27,1	27,6	28,2	28,8	29,5	30,2	30,9	31,6	32,6	33,5
25	[illegible]	22,3	22,6	22,9	23,2	23,6	24,0	24,4	24,8	25,3	25,9	26,4	26,9	27,6	28,3	29,0	29,7	30,4	31,4	32,3
I — 0	20,7	21,0	21,3	21,6	21,9	22,3	22,7	23,1	23,6	24,0	24,5	25,0	25,6	26,2	26,9	27,6	28,4	29,1	30,0	30,9
5	19,3	19,6	19,9	20,2	20,5	20,9	21,3	21,7	22,1	22,6	23,1	23,6	24,1	24,7	25,3	26,0	26,8	27,6	28,4	29,3
10	17,9	18,1	18,4	18,7	19,0	19,3	19,7	20,1	20,5	21,0	21,5	22,0	22,5	23,0	23,6	24,3	25,1	25,8	26,6	27,5
15	16,3	16,5	16,8	17,1	17,4	17,7	18,1	18,4	18,8	19,2	19,7	20,2	20,6	21,2	21,8	22,5	23,2	23,8	24,6	25,4
20	14,7	14,9	15,1	15,4	15,7	16,0	16,3	16,6	17,0	17,4	17,9	18,3	18,7	19,2	19,8	20,4	21,1	21,7	22,4	23,2
25	13,0	13,2	13,4	13,6	13,9	14,1	14,4	14,8	15,2	15,5	15,9	16,2	16,6	17,1	17,7	18,2	18,8	19,4	20,0	20,7
II — 0	11,3	11,4	11,6	11,8	12,0	12,2	12,5	12,8	13,1	13,4	13,8	14,2	14,4	14,9	15,5	15,9	16,3	16,9	17,5	18,1
5	9,5	9,6	9,7	9,9	10,1	10,3	10,5	10,8	11,0	11,3	11,6	11,8	12,1	12,6	13,1	13,4	13,8	14,3	14,8	15,4
10	7,6	7,7	7,8	7,9	8,1	8,3	8,5	8,7	8,9	9,1	9,4	9,5	9,7	10,1	10,6	10,9	11,2	11,6	12,0	12,5
15	5,7	5,8	5,9	6,0	6,1	6,2	6,4	6,6	6,8	6,9	7,1	7,2	7,3	7,6	8,0	8,2	8,5	8,8	9,1	9,4
20	3,8	3,8	3,9	4,0	4,1	4,2	4,3	4,4	4,6	4,6	4,8	4,9	4,9	5,1	5,4	5,5	5,7	5,9	6,1	6,3
25	1,9	1,9	2,0	2,0	2,1	2,1	2,2	2,2	2,3	2,3	2,4	2,5	2,5	2,6	2,7	2,8	2,9	3,0	3,1	3,2
III + 0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
5	1,9	1,0	2,0	2,0	2,1	2,1	2,2	2,2	2,3	2,3	2,4	2,5	2,5	2,6	2,7	2,8	2,9	3,0	3,1	3,2
10	3,8	3,8	3,9	4,0	4,1	4,2	4,3	4,4	4,6	4,6	4,8	4,9	4,9	5,1	5,4	5,5	5,7	5,9	6,1	6,3
15	5,7	5,8	5,9	6,0	6,1	6,3	6,4	6,6	6,8	6,9	7,1	7,2	7,4	7,6	8,0	8,2	8,5	8,8	9,1	9,4
20	7,6	7,7	7,8	7,9	8,1	8,3	8,5	8,7	9,0	9,2	9,4	9,5	9,7	10,1	10,6	10,9	11,2	11,6	12,0	12,5
25	9,4	9,5	9,7	9,8	10,0	10,2	10,5	10,8	11,1	11,3	11,6	11,8	12,0	12,5	13,1	13,4	13,8	14,3	14,8	14,3
IV + 0	11,2	11,3	11,5	11,7	11,9	12,1	12,5	12,8	13,1	13,4	13,7	14,0	14,4	14,9	15,4	15,8	16,3	16,8	17,4	18,0
5	12,9	13,1	13,3	13,5	13,7	14,0	14,4	14,7	15,0	15,4	15,7	16,2	16,6	17,1	17,6	18,1	18,7	19,3	19,9	20,6
10	14,6	14,8	15,0	15,2	15,5	15,8	16,2	16,5	16,9	17,3	17,7	18,2	18,7	19,2	19,7	20,3	20,9	21,6	22,3	23,0
15	16,2	16,4	16,6	16,9	17,2	17,6	18,0	18,3	18,6	19,1	19,5	20,0	20,6	21,1	21,7	22,3	23,0	23,7	24,4	25,2
20	17,7	17,9	18,2	18,5	18,8	19,2	19,6	19,9	20,3	20,8	21,3	21,8	22,4	22,9	23,5	24,2	24,9	25,6	26,4	27,2
25	19,1	19,4	19,7	20,0	20,3	20,7	21,1	21,4	21,8	22,3	22,9	23,4	24,0	24,6	25,2	25,9	26,6	27,3	28,1	29,0
V + 0	20,5	20,8	21,1	21,4	21,7	22,1	22,5	22,9	23,3	23,8	24,3	24,8	25,4	26,1	26,7	27,5	28,1	28,9	29,7	30,6
5	21,7	22,0	22,4	22,7	23,0	23,4	23,8	24,2	24,5	25,1	25,6	26,1	26,7	27,4	28,1	28,8	29,4	30,2	31,1	32,0
10	22,9	23,2	23,5	23,8	24,2	24,5	24,9	25,3	25,8	26,2	26,7	27,2	27,8	28,5	29,3	30,0	30,6	31,4	32,3	33,2
15	24,0	24,2	24,5	24,8	25,2	25,5	25,9	26,3	26,8	27,2	27,7	28,2	28,8	29,5	30,3	31,0	31,6	32,4	33,2	34,1
20	24,9	25,1	25,4	25,7	26,1	26,4	26,8	27,2	27,7	28,1	28,6	29,1	29,7	30,4	31,1	31,8	32,4	33,2	34,0	34,9
25	25,7	25,9	26,1	26,4	26,8	27,1	27,5	27,9	28,4	28,8	29,3	29,8	30,3	31,0	31,7	32,3	33,0	33,7	34,5	35,4
VI + 0	26,3	26,5	26,7	27,0	27,4	27,7	28,1	28,5	28,9	29,3	29,8	30,3	30,8	31,4	32,1	32,7	33,4	34,1	34,9	35,8
5	26,7	26,9	27,1	27,4	27,8	28,1	28,4	28,8	29,2	29,6	30,1	30,5	31,0	31,6	32,3	32,8	33,5	34,3	35,0	35,8
10	27,0	27,2	27,4	27,7	28,0	28,3	28,6	28,9	29,3	29,7	30,2	30,6	31,1	31,7	32,3	32,8	33,5	34,3	35,0	35,7
15	27,0	27,2	27,4	27,7	28,0	28,3	28,6	28,9	29,2	29,6	30,1	30,5	31,0	31,5	32,0	32,5	33,2	33,9	34,6	35,3
20	26,9	27,1	27,3	27,5	27,8	28,1	28,4	28,6	28,9	29,3	29,5	30,2	30,6	31,0	31,4	32,1	32,8	33,4	34,1	34,7
25	26,6	26,7	26,9	27,1	27,3	27,6	27,9	28,1	28,4	28,9	29,2	29,6	29,9	30,3	30,8	31,4	32,0	32,6	33,2	33,8
VII + 0	26,1	26,1	26,3	26,5	26,7	26,9	27,2	27,4	27,8	28,1	28,4	28,7	29,0	29,4	29,9	30,4	31,0	31,6	32,1	32,7

The inftruments ufed in making the preceding obfervations were :

1. A clock, made by Mr. Ellicot, with an apparatus for correcting the effects of heat and cold ; the fame which Meffieurs Mafon and Dixon had to the Cape of Good Hope in the year 1761.

2. An aftronomical quadrant, made by Mr. Bird, of one foot radius.

3. Two reflecting telefcopes, of two feet focus, made by Mr. Short; and a divided object-glafs micrometer, made by the fame gentleman, of 501,45 inches focal length.

We ufed the micrometer with a magnifying power of 60; the contacts of Venus with the Sun's limb were obferved with a magnifying power of 120, and all the other obfervations with one of 90.

Both the thermometers, ufed in the preceding obfervations, were according to Fahrenheit's fcale; and the characters + and —, which are annexed to their altitudes, denote that they ftood fo many degrees above or below the cypher refpectively: where neither of thofe characters appears, the number is to be underftood above the cypher.

LXVI. *Extract*

LXVI. *Extract from the Journals of the* Royal Society, June 23, 1768, *respecting a Letter addressed to the Society by a Member of the House of* Jesuits *at* Pekin *in* China; *by* Charles Morton, *M. D. Sec. R. S. and Fellow of the Imperial Acadd. Natur. Curios. &* Petropol. *and of the Royal Academy of* Gottingen.

THIS letter, the original of which is in the French language, consists of 28 pages in close folio; to which are subjoined 44 pages of notes; and 27 pages of drawings, to which the letter and notes refer for illustration.

It relates to some disquisitions of Mr. Turberville Needham, F. R. S. concerning a supposed connection between the hieroglyphical writing of antient Egypt, and the characteristic writing which is in use at this day, amongst the Chinese.

Divers of the Society remember Mr. Needham's tract upon this subject, which was printed at Rome in the Latin tongue, 1761, addressed to this and the Antiquarian Societies.

This conjecture of Mr. Needham's, pregnant with so many curious consequences, engaged the attention of the Literati of Europe: the generality wishing

ſucceſs to it; and divers, either from a particular information, or for other reaſons, oppoſing it. Mr. Deſguignes of Paris, F. R. S. Mr. Bartoli of Turin, antiquary to the King of Sardinia; the late Abbé Winkelman, antiquary to the Pope, and Mr. Montagu, F. R. S. were the principal of thoſe who thought themſelves concerned to oppoſe Mr. Needham; and what they have been pleaſed to communicate, either in print or manuſcript, has been already laid before the Society; and the laſt gentleman, viz. Mr. Montagu, has alſo ſent to England a caſt of the buſt of Turin, inſcribed with certain characters, which gave occaſion to Mr. Needham's conjectures: which caſt, by the bounty of his Majeſty our Patron, is now in the Britiſh Muſeum.

The ſubject in queſtion ſeemed ſufficiently intereſting to ſeek an anſwer from the only competent judges, the literati of China; and your Secretary, firſt by the encouragement of Thomas Hollis, Eſq; F. R. S. and ſubſequently by the aſſiſtance of Thomas Wilcocks, Eſq; F. S. A. and the particular favour of the Directors of the Eaſt-India Company, has at length obtained it.

In order to this, a letter was written, in conjunction with Mr. Alban Butler, late of Pall-Mall, (who had ſome intereſt among the Jeſuits at Pekin) ſtating the matter in queſtion, and deſiring the favour of an anſwer; which anſwer is the letter that has been read to the Society.

The particulars which were ſtated to the Jeſuits at Pekin, and have been recited to the Society, were as follows, viz. 1. Whether certain characters, to the number of 29, copied from the buſt at Turin, together

ther with divers other characters, to the number of 200, copied from undoubted monuments of Egypt, are really and indeed Chinese characters; and if they be, of what dialect, and of what age are they?

2. What sense doth each of these characters express; and what is the particular interpretation?

3. Doth the history of China, or popular tradition, or any analogy with the modern or antient method of writing of any other nation, afford ground for supposing that these characters have been received from foreigners; or were they invented by the Chinese themselves?

4. Are there any monuments or customs amongst the Chinese, which resemble those of the antient Egyptians; or which should induce us to think, that there has ever been any communication between the two nations?

The answer received from China takes notice only of the small number of characters which were copied from the bust of Turin; occasioned probably by some accident or failure in the pacquets, of which there were three copies sent, and one of them containing the Turin characters only; the answer is dated from Pekin, October 20, 1764, addressed to the Members of this Society, but with no subscription, or signature, excepting four stars, and this addition of *the company* of *Jesus*.

The author's method, or order, is as follows:

1. An introductory preface. 2. A state of the enquiry, as collected partly from the letter, and partly from Mr. Needham's printed book. 3. What the author calls an historical picture of the Chinese tongue and its characters. 4. An applica-

tion of this hiſtorical delineation, in the way of principles, to decide concerning the 29 characters of the buſt of Turin. 5. A more general application of the ſaid principles, in order to elucidate the hieroglyphical writing, and conſequently the antiquities of Egypt, by a propoſed collation with the antient ſymbolical writing of China, exemplified by divers inſtances: and laſtly the notes, containing circumſtantial details of ſome particulars, as well hiſtorical as critical, which might otherwiſe have broken the thread of the letter.

I ſhall not pretend to give an adequate idea of this curious paper, within the compaſs of an extract ſuited to this place.

The particular branch diſcuſſed in this letter, as well as the general learning of China, are ſubjects in a manner new to Europe; and the various books of the Chineſe are called by the author a Potoſi, which might enrich Europe; eſpecially with regard to laws, government, the uſeful arts, natural hiſtory, and the like. Some ſtrictures from the letter are to this effect: and,

1. In the preface mention is made of the inſufficient attempts of the Greeks and Romans to explain the hieroglyphical writing of Egypt; and of the later attempts of father Kircher, and Mr. de Mairan, who anticipated Mr. Needham in the idea of explaining them by the characteriſtic writing of China; which idea they quitted almoſt as ſoon as they had formed it.

2. The ſtate of the queſtion is expreſſed as follows: "Mr. Needham has obſerved, that the ſym-
"bols or hieroglyphical characters of the Iſis of Tu-
" rin,

" rin, appear like ſeveral Chineſe characters, ſuch as
" they are found in the great dictionary Tching,
" tſee, tong: upon which he conjectures, firſt, that
" the Chineſe characters are the ſame in many re-
" ſpects, as the hieroglyphics of Egypt; and ſecond-
" ly, That one may be able to diſcover the ſenſe of
" hieroglyphics by the comparative and appropriated
" ſignification of the Chineſe characters."

The author, thirdly, having mentioned the difficulty of rendering himſelf intelligible to the literati of Europe, for want of a certain acquaintance with proofs of fact and hiſtory, criticiſm and grammar, proceeds to the hiſtorical detail, which is the ſubject chiefly enlarged upon by him. He notes the enthuſiaſm of Voſſius for the antiquity of the Chineſe, and the rage of Renaudot againſt it; and then declares his own opinion, that they have ſubſiſted as a nation, from the time of the great emigration which followed the confuſion of tongues. He dates the antiquity of Egypt from the ſame epoch, and gives reaſons, particularly in the notes, for the probability of their different routs. He enquires into the uſe of writing; and declares his opinion, that it was already eſtabliſhed in the antediluvian world; and might be derived in common to the two nations in queſtion. He makes light of any ſuppoſed variation of it, at the confuſion of tongues: and wiſhes that Mr. Needham had expreſſed his own opinion concerning the commencement of it. He affirms, that there is not the leaſt mark or trace now remaining of any ſubſequent communication between the Chineſe and Egyptians. But whether our author's opinion of the origin of writing, or the contrary one, of each nation having invented its

its own, be adopted: he candidly owns, that any connection between the two modes of writing, is hardly difcernible at this day. He affirms, that the Chinefe language is one of the moft antient; and perhaps the only one which has been fpoken without interruption; and is yet a living language; the fmall number and the fhortnefs of its words having fo guarded it from changes, that they could fcarcely extend farther than the pronunciation.

They diftinguifh in the Chinefe language, 1ft, the Kou-ouen, the language of the *King*, and other books written in this tafte. The harangues of the *Chou-king*, and the fongs of the *Chi-king*, prove that it was fpoken formerly. It is prodigioufly laconick. 2dly, The *Ouen-tchang*, the language of *relevees*, elevated compofitions, and books. This language, excepting fome proverbs, axioms, and forms of compliment, is no longer ufed in fpeaking. 3dly, The *Kouan-hoa*, the language of men in office. This is the only language fpoken at court, and in good company, and ufed in books; and this alone runs through the empire. 4thly, The *Hiang-tan*, patois or provincial jargon. Each province and town, and almoft every village has its own. In fpight of thefe varieties, the Chinefe tongue counts but about 330 words. From hence the Europeans conclude, that it is barren, monotone, and hard to underftand. But they ought to know, that the four accents called *ping*, uni *(even)* *chung*, élevé, *(raifed)*, *kiu* diminué *(leffened)*, *jou*, rentrant, *(returning)*, multiply almoft every word into four, by an inflexion of voice which it is as difficult to make an European comprehend, as it is for a Chinefe to comprehend the fix pronunciations of the French E.

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Theſe accents do yet more; they give a certain harmony, and pointed cadence, to the moſt ordinary phraſes: with regard to clearneſs, let fact decide. The Chineſe ſpeak as faſt as we do, ſay more things in fewer words, and underſtand one another.

The Chineſe have no diſtinct knowledge of the invention of writing; as our author proves by quotations from their moſt antient books; which are involved in fable, and contradict each other. One of them indeed mentions this curious particular; that Fou-hi, by introducing the eight Koua, or elementary characters, put an end to the uſe of knots upon cords, for the purpoſes of government; which ſeems to be analogous to what has been obſerved in America.

The author defines the Chineſe characters, according to his conception of them in their origin, to be images and ſymbols which ſpeak to the mind by the eyes. Images, for ſenſible things; ſymbols, for mental. Images and ſymbols which are tied to no ſound, and may be read in every tongue. The book *Tſee-hio-leang-tſin* divides the characters into ſix ſorts, *Lieou-y*. The *firſt*, called *Siang hing*, ſhape, image, is a true picture of ſenſible things. Thus one ſees in the antient characters, trees, birds, vaſes, &c. rudely traced out.

The 2d, called *Tchi-che*, indication of the thing, is made by an addition to the ſhape, or to the ſymbol, which puts the thing that one would expreſs before the eyes. For example, the character of *ſmall* placed over that of *great*, to ſignify *pyramidal, terminated in a point*. The 3d, called, *Hoei-y*, *junction of idea, aſſociation*, conſiſts in joining two characters, to expreſs a thing which neither the one nor the other ſignify ſeparately.

ſeparately. For example; the ſhape of *mouth* placed aſide that of *dog*, to ſignify the verb *bark*. The 4th, *Kiai-in*, *explication*, or expreſſion, of *the ſound*, owes its origin to the difficulty of tracing in a manner ſufficiently diſtinct, all the ſorts of fiſhes, animals, vaſes, trees, &c. To ſupply this, they contrived to place the ſimple character of one ſound on the ſide of the figure. For example; the character of the ſound *y a* on the ſide of the figure of a bird, to ſignify a *duck*: the character of *ngo*, to ſignify a gooſe.

The 5th, called *Kia-ſie*, idea borrowed, metaphor, hath opened an immenſe field to the invention of characters; or rather, to the manner of making uſe of them. In effect, by virtue of the *Kia-ſie*, one character is ſometimes taken for another; choſen to expreſs a proper name; turned aſide to a ſenſe allegorical, metaphorical, ironical; and puſhed even to an antiphraſis, in giving it a ſenſe oppoſite to that wherein it is employed elſewhere. It muſt be owned, that this 5th claſs gives the Chineſe tongue a force, and a vivacity of colouring, that no other tongue can attain.

But it is alſo one of the principal cauſes of its obſcurities. The figurative ſenſe of a character has not always a due analogy with the proper ſenſe.

The 6th, called *Tchouen-tchou*, developement, explication, conſiſts only in extending the primitive ſenſe of a character, or in making detailed applications of it. Thus the ſame character is ſometimes verb or adverb, ſometimes adjective or ſubſtantive. Thus again, the character *ngo*, which ſignifies *evil*, ſerves to expreſs *hatred*, *to hate*, *miſhapen*, &c.

Theſe ſix, *Lieou-y*, ſuch as here deſcribed, are as it were the ſources from whence flow all the characters

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in a manner equally ſimple, clear, and natural; and the whole number of characters is thus increaſed from 300, to 80,000. And, the author ſubjoins, "One "muſt read the fine paſſages of the *King*, to com- "prehend what force, grace, energy, amenity, "grandeur, and ſimplicity, the Chineſe characters "have, where they are well aſſorted, and well con- "nected.

"I would willingly define the Chineſe characters "to be the pictureſque algebra of the arts and ſci- "ences. In truth, a phraſe of good ſtyle is as diſem- "baraſſed of every thing that is intermediary, as the "cloſeſt algebraic demonſtration."

Unleſs one were to give the lye to the Chineſe, and to the ſmall number of the characters of antient times which they have preſerved; it is not poſſible to deny, that they did, in the moſt remote antiquity, make uſe of ſhapes, or likeneſſes of ſenſible things, and of ſymbols to form their characters, nearly in the taſte of the hieroglyphics of Egypt: and one need but caſt the eye on ſome of the characters which are copied in the pages 5, 6, and 7, (TAB. XXIV, XXV, XXVI.) of the plates following, to be convinced thereof. But had not the Chineſe, even from that time, the art of contracting theſe figures, and reducing them to ſome ſtrokes or lines, by analyſis and abbrevia- tion? To judge thereof by ſome of the antient characters, it appears, that the Chineſe did re- duce ſeveral to certain ſtrokes ill enough aſſembled; probably for the conveniency of writing. And whenſoever the time was wherein the abbreviations began, they were neceſſary; 1ſt, becauſe without them, writing would have been too difficult: 2dly, becauſe one muſt have had volumes to convey a very

ſmall matter. In effect, without being well verſed in drawing, how could one trace in an agreeable manner ſo many figures and ſymbols? The difficulty augments when one reflects, that a good many characters were compoſed of divers ſymbols and images, the reduction of which ought to be very well touched, not to be diſagreeable; eſpecially near to other characters that were leſs compound. It is natural to think, that they would not make uſe of images and ſymbols intire, and traced in their juſt proportion, but for great monuments, where room was not wanting. And yet it ſhould not be denied, that they had recourſe to the analyſed characters, for certain places leſs advantageous.

The fact eſtabliſhed by what remains of the Chineſe monuments is, that the ſhapes and the ſymbols have paſſed from a contour ſufficiently regular, to ſome lines oddly aſſembled; and that the lines themſelves have been yet decompounded, and melted, into theſe ſix lines,  out of which, at this day, are compoſed all the characters in uſe. The ſimpleſt are made of one or two of theſe lines; and they count as far as 20 or 30, or more lines, in the more compound characters. To avoid the confuſion and obſcurity which this great abbreviation would have cauſed, they have fixed the number of the lines of the characters which repreſent the 200 elementary images and ſymbols ſpoken of. Theſe abbreviations thus fixed are called *Pou*, *Claſſes* or *Tribunals*, as Mr. Fourmont tranſlates. For example; the *Pou* of man, of woman, of trees, of diſeaſes, of great, of ſmall, of vaſe, &c. In brief, for greater clearneſs, and to range the characters in the dictionaries, there is in each charac-

ter

ter a diſtinctive or differencing *Pou*, which predominates, and under which the character is placed. This differencing *Pou* is the part of the character which hath moſt influence in its ſignification; ſaving the exceptions, and oddities, from which the Chineſe is no more exempt than other tongues. A bare inſpection into the dictionary *Tching-tſee-tong*, will render theſe details intelligible.

The misfortune, and a very great misfortune, of the Chineſe characters is, that theſe abbreviations have been made by little and little, in different places, and without rule: ſo that there are characters which have been abridged, or more properly truncated, and disfigured a very great number of ways: and the moſt part, ſo much, as to be no longer knowable by the primitive form. To give ſome idea of this, the author has cauſed to be copied the variations of four characters (ſee the plates 7, 8, 9. Tab. XXVI, XXVII, XXVIII.); and one may judge by this ſample, how frightfully disfigured muſt be thoſe characters which are woven out of ſeveral other characters. For the different characters which are thus united to make one only, are curved, lowered, lengthened, drawn in, or contracted, to the end that each line may be ſo placed, as that all together may make the contraſt of a ſimple character, and occupy no more ſpace than it does. A like conſtraint ought to disfigure many of the elementary characters which are joined together to make one only. But when we add thereto the abbreviations and various readings, it is clear that they can no longer be knowable by their primitive characters. And this, to obſerve it *en paſſant*, is one of the reaſons which has rendered the edition of the *King* under the *Han* ſo difficult, and

perhaps is the principal caufe of their obfcurity. In effect, the primitive images and fymbols being altered, how can one find the fenfe of them? It is no more according to the rule of the *Lieou-y*. The decompofition of the elementary characters whereof it is compofed, no longer gives its true analyfis. The more one feeks the fenfe which ought to refult from their affemblage, the farther one is from it: becaufe that this affemblage is not the true one. It is as if one fhould read (in French) *delires* for *delices*. This change of the *c* into *r* fubfifting, all the fignifications that one fhall feek to *delires*, will never arrive at the idea prefented by *delices*.

If the comparifon is lame, it is becaufe that it reprefents not fufficiently clearly how far a Chinefe character feparates from its true fignification, by the alteration of fome one of the lines that compofe it. The deftruction of the books by fire has rendered the evil without a remedy. When peace was reftored to letters, they fpared neither care nor inquiry to recover the *King*, and other antient books. But few copies having efcaped the flames, and thofe not in the beft prefervation, they were deprived of the great advantage to be drawn from collations, to difcover the primitive characters. Writing had changed; tradition was almoft extinguifhed. It was neceffary to be learned, even to decypher the manufcripts: how fhould they be able to purfue the difcuffion fo far as the various readings; and unravel, amongft abbreviations almoft unknown, the true fymbols and likenefs of which a character was woven. The editors were not fparing of their labour herein; but each had his fyftem, and his conjectures. Who would venture

ture to ſay, that the edition which has prevailed has not many miſtaken characters? and let it be even the beſt, learned men, who have laboured ſince in the analyſis of the characters, are not agreed amongſt themſelves; and they bring each reaſons capable of ſuſpending the judgement of critiques. This variety of opinion hath cauſed much variety in the *orthography*, if one may ſo call the manner of writing a character with ſuch or ſuch a *Pou*. The manner accordingly has been floating and uncertain, for very many characters, until the great dictionary *Kang-hi-tſe-tien*, which has fixed it.

The author winds up this curious detail with the following remark, which he ſays is eſſential. All that has been ſaid of the various readings and abbreviations of the characters is independent of the five ſorts of writing ordinarily counted by lettered men. The firſt is called *Kou-ouen* (ſee plates the 5th, 6th, 7th, and 8th, and part of the 9th, TAB. XXIV—XXVIII.) This is the moſt ancient form of writing; and there remains now hardly any more traces of it. The ſecond, *Tchoang-tſee*, (alſo read *Tchouen-tſee*, vid. plate 1. TAB. XIX.) has ſucceeded the *Kou-ouen*; and has laſted even to the end of the Dynaſtie of the Tcheou. It was this which was in uſe from the time of Confucius, and of which the abbreviations and various readings have been moſt fatal. The third, *Li-tſee*, (ſee plate 2. TAB. XXI.) began under the reign of *Chi-hoang-ti*, the founder of the Dynaſtie of the Tſin, and the great enemy of letters and of lettered men. The fourth, *Hing-chou*, is deſtined for impreſſion, as with us the Roman and Italic. (See plate 3. TAB. XXII.)

The

The fifth ſort, *Tſao-tſee*, (ſee plate 4. TAB. XXIII.) was invented under the *Han*, and would have deſtroyed every thing, if it had prevailed. It is a ſort of writing with the ſtroke of a pencil, with a very light and well-experienced hand: but it disfigures the characters beyond expreſſion. It has no courſe, but for the preſcriptions of phyſicians, prefaces of books, inſcriptions of fancy, &c.

To return to the various readings, and abbreviations; although it be true that theſe different ſorts of writing have augmented the number of them; nevertheleſs the three laſt have done no great harm; becauſe they have been directed by learned men, conſecrated by publick authority, and bear more on the general form of the characters, than on their orthography. Thus the literati do not complain, further than their having cauſed the loſs of the antient characters, which it would have been well to conſult, to have had the true analyſis of ſeveral of the characters of this day, which they think ill written, and disfigured.

And thus, at length, having compleated his hiſtorical detail, (which I have here repreſented very imperfectly) our author decides concerning Mr. Needham, viz. that the characters of the buſt of Turin, (though four or five of them, viz. N° 2, 3, 8, 9, 31, have a ſenſible reſemblance to the like number of characters in the Chineſe dictionary), are not genuine Chineſe characters; having no connected ſenſe, nor a proper reſemblance to any of the different forms of writing; indeed the whole inſcription has nothing of Chineſe in the face of it. As a farther proof, our author took the opinion of divers of the Chineſe literati, whoſe province it is to ſtudy the antient writings;

tings; who all declared the ſame thing; and that they did not underſtand them, nor had ever ſeen the like of them.

It is owned, however, that, according to the Chineſe interpretation of the five reſembling characters, they are ſimple ideas, or ſymbols, not characterized by the farther circumſtantiating lines; and are, without coherence, in the way of Nomenclator.

But finally, to enable the Society to judge for themſelves; our learned correſpondent has ſent a collection of very antient inſcriptions, above one hundred in number, which may be compared with the inſcription of Turin; as alſo, ſome drawings of vaſes, and other antiquities. See plates, from 13 to 27 (TAB. XXII to XLVI.) incluſive. The particular matter of enquiry, viz. the characters of the buſt of Turin being thus diſpoſed of, our author, who is againſt renouncing Mr. Needham's general conjecture, without farther examination, as it may notwithſtanding conduct to many diſcoveries, applies himſelf, fifthly, to a farther and more general inveſtigation, by an actual collation of ſuch Egyptian hieroglyphics as do undoubtedly reſemble antient characters, yet remaining amongſt the Chineſe: in order to which, he has given us drawings of 73 ſuch hieroglyphics, collected chiefly from Kircher (as he had no better materials), and has placed by them the correſponding Chineſe characters, (ſee plates, from 9 to 12, TAB. XXVIII to XXXI incluſive) both anient and modern. He is ſufficiently diffuſe and curious, in two or three examples, to point out the method and moſt intereſting ſubjects of enquiry, viz. the leading notions concerning the Deity, and the religion of the primitive times; and he alſo deſcribes the properties

of

of the ſymbolical animals, which are ſuppoſed to be ſignificant of the rational and moral qualities; but enters a caution againſt theſe, as being, moſt likely, the invention of later times. He argues ſtrenuouſly for the early and uninterrupted Theiſm of the Chineſe; and concludes with an apology for the condition of a miſſionary, the duties of whoſe profeſſion, and ſeparation from divers neceſſary means of information, render him, in his own opinion, very unfit for literary inquiries.

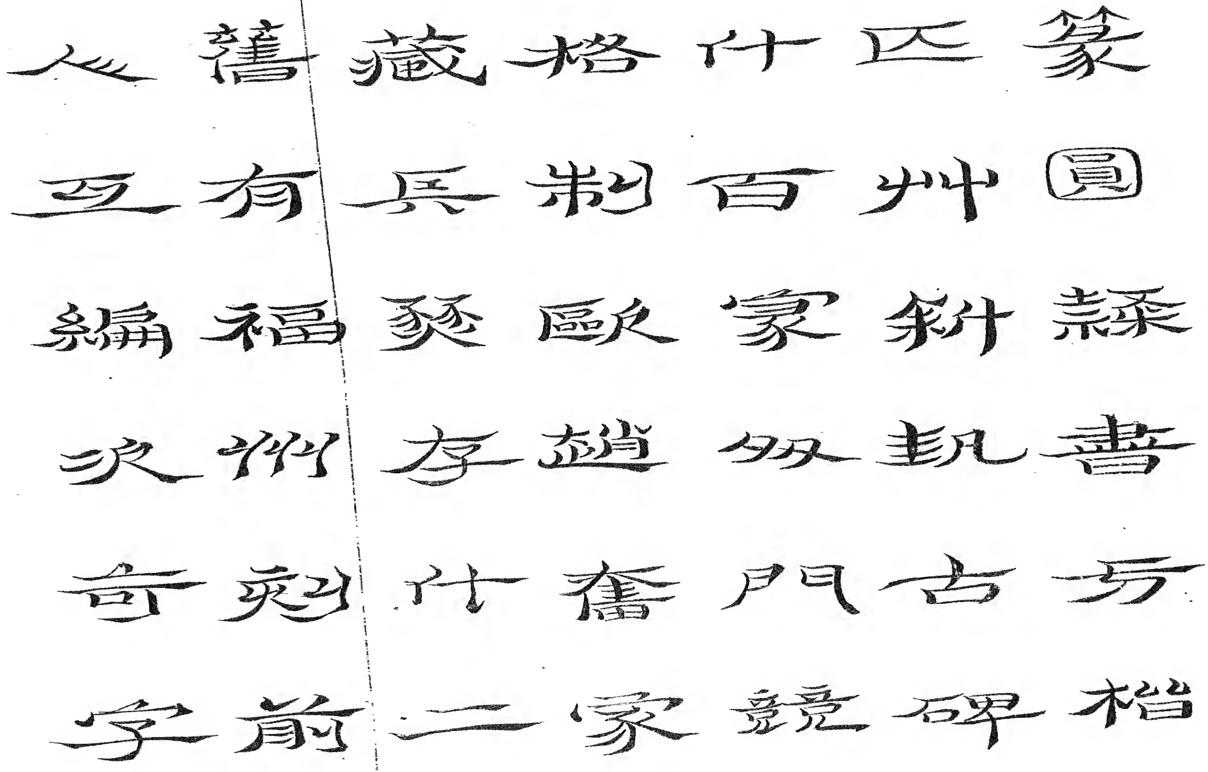
C. Morton.

LXVII. *Obſer-*

The character called Tchouen Tsée.

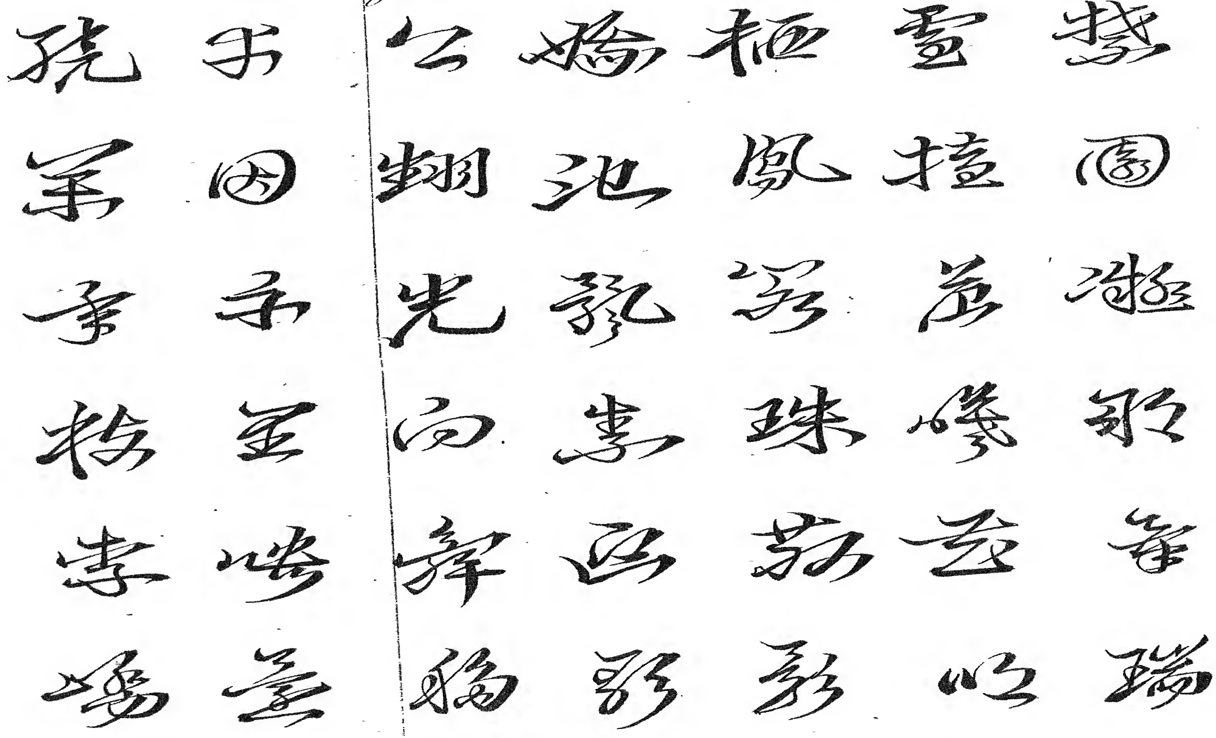
J. Mynde sculp.

The character called Li Tsée.



The character called Hing Chou.

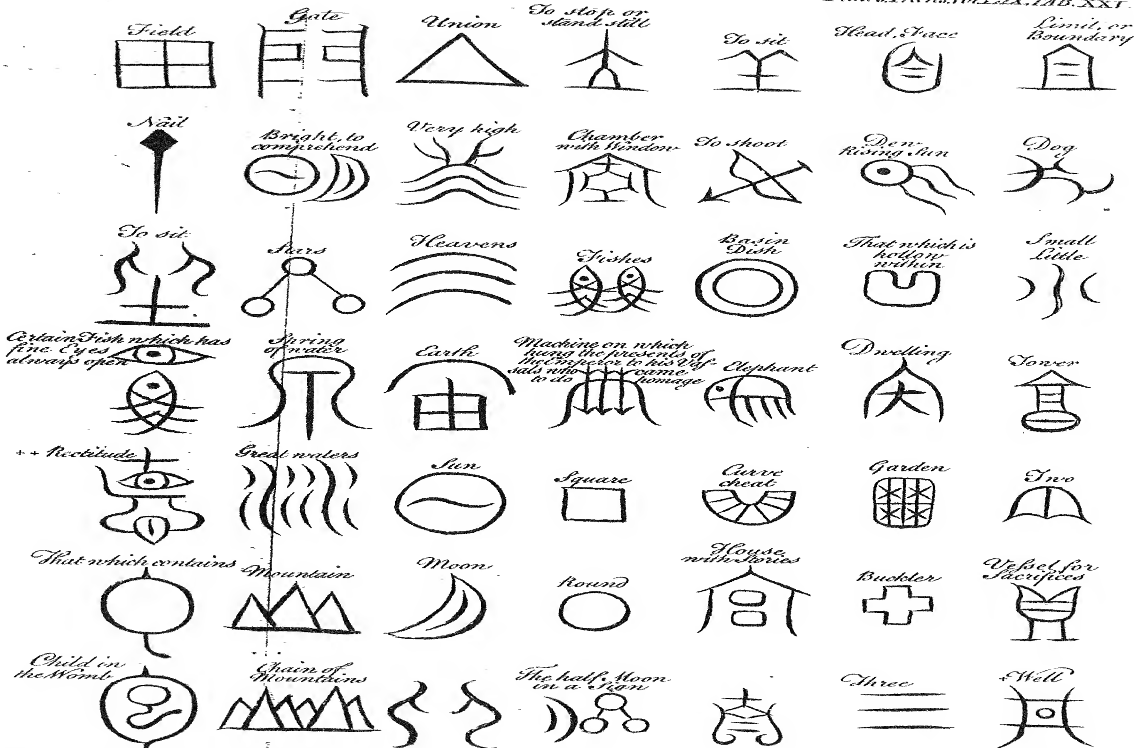
The vulgar character, called Tsao Tsée.



Kou ouen.

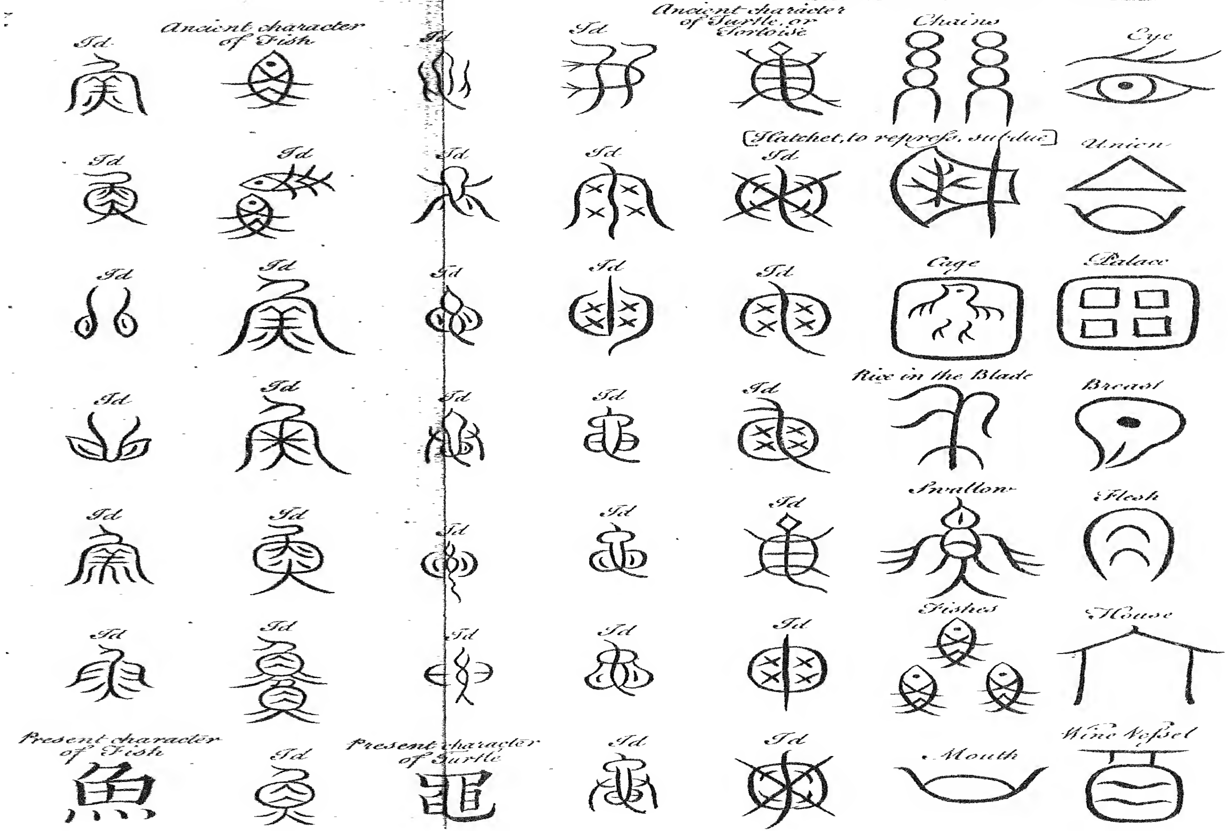
This is the most ancient Writing.

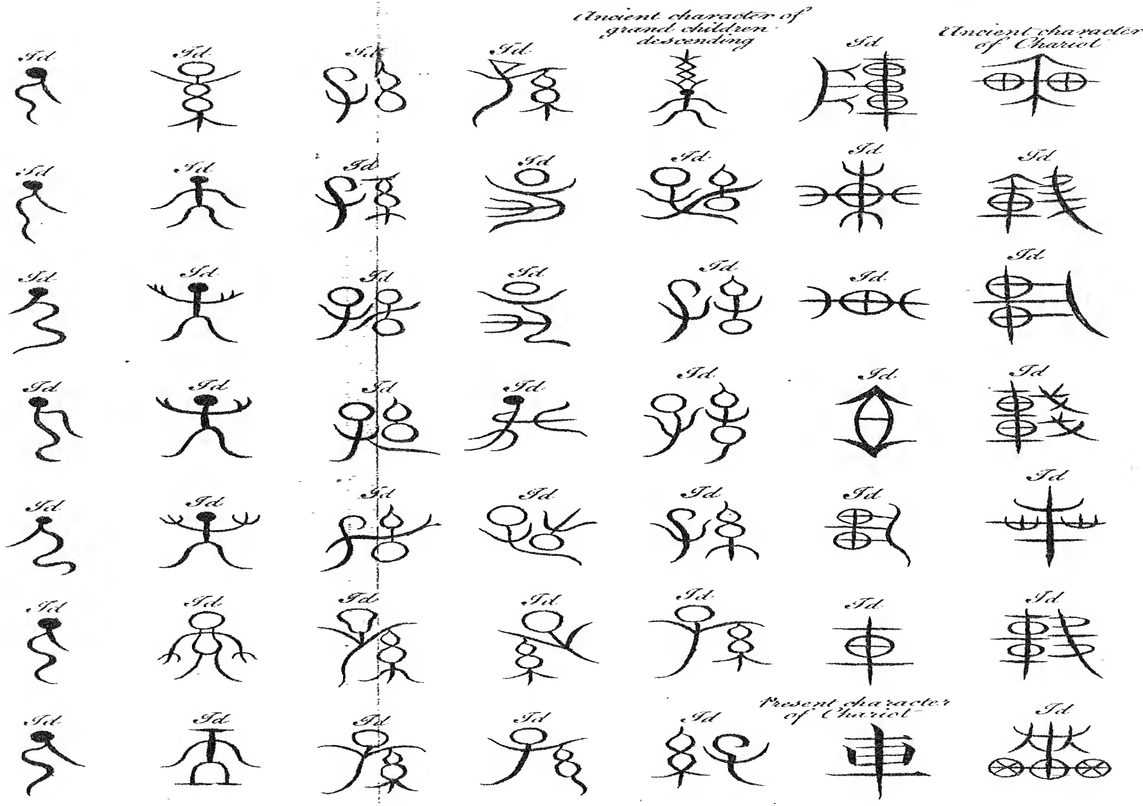
This letter is a true likeness of the Chinese burying places: round are trees; & in the middle, an Oval Solid, or Ba on which is raised the Pyramid of Earth which is over the Coffin.
It is singular, that the figure (or character) of Dragon or Serpent, should have in it the two figures ʌ.ʌ. which are the abridged figures of Man, why two Men over against the Serpent.



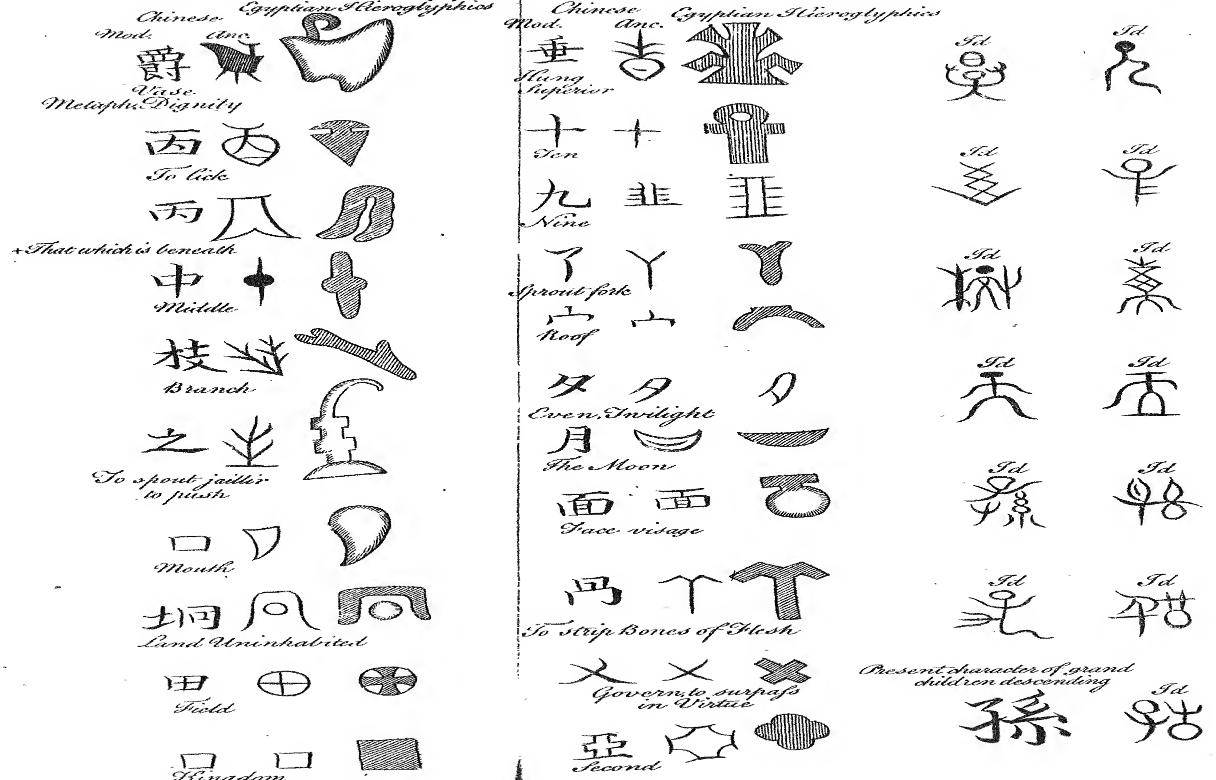
\+ This is still the form of the aperture of the Chinese Wells. The Mouth is very small: that the Women may not throw themselves in, from Despair.

++ This character is composed of which signifies to obey Heaven; & of which signifies Heart. is composed of + ten, cross, Eye. & which signifies Loss, Death.


Ancient character of Fish
Id
Present character of Fish
Present character of Turtle
Ancient character of Turtle, or Tortoise
[Hatchet, to repress, subdue]
Chains
Eye
Union
Cage
Palace
Rice in the Blade
Breast
Swallow
Flesh
Fishes
House
Mouth
Wine Vessel


Ancient character of grand children descending
Ancient character of Chariot
Present character of Chariot
Id

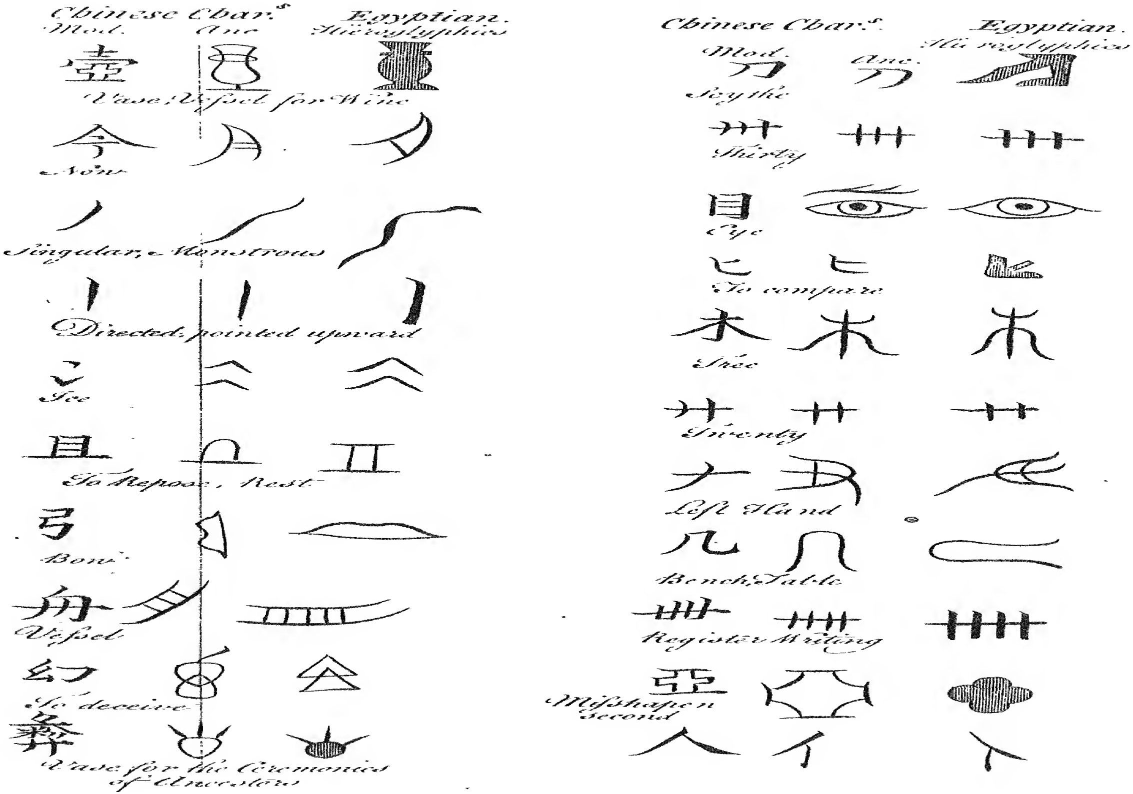
Chinese characters approaching to some Hieroglyphics of Egypt



+ This is spoken of the Earth, with respect to the Heavens.


Continuation.
Chinese Char.s Mod. Anc.
Egyptian Hieroglyphics
方
Square
卜
Divination
圓
Round
凹
Hollow within
口
Mouth
五
Five
合
Union
夕
Evening
且
Immoveable
合
Union
Chinese Ch.rs Mod. Anc.
Egyptian Hieroglyphics
五
Five
月
Moon
月
Moon
其 厶
Concupiscence, to Intrigue
工 工
Work
淵
Deep, profound
節
Knot, or Brace of a Drum. Metaph. Temperance
丹 丹
Flesh Colour
杵
Pestel of a Mortar
草
Grass

Continuation.



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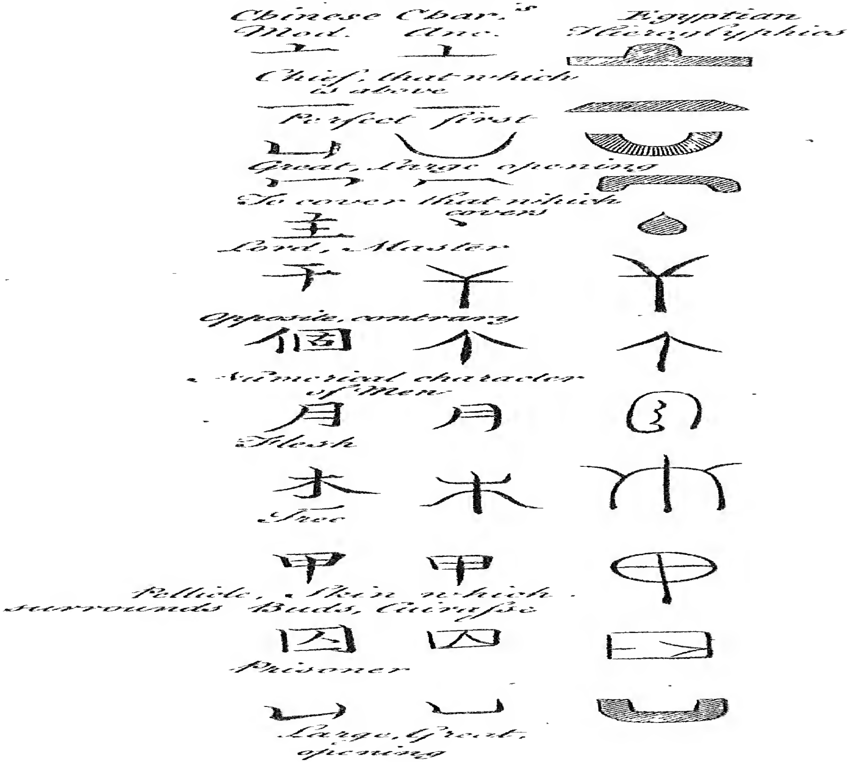
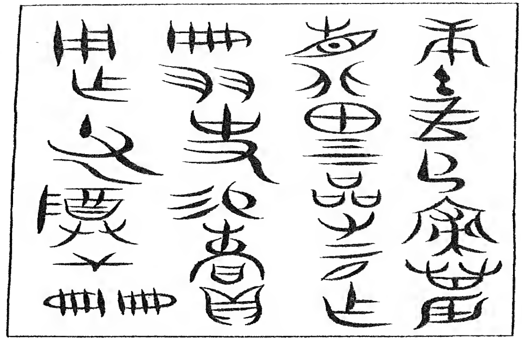
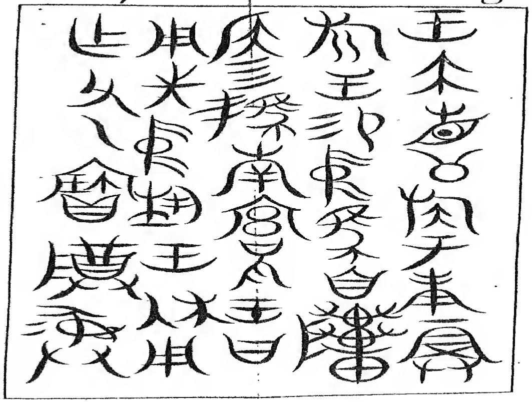
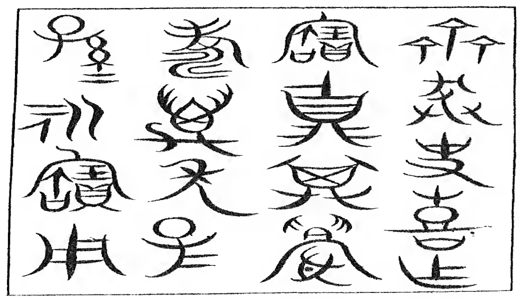


Inscriptions which are thought to be of the Dynasty of the Chang.

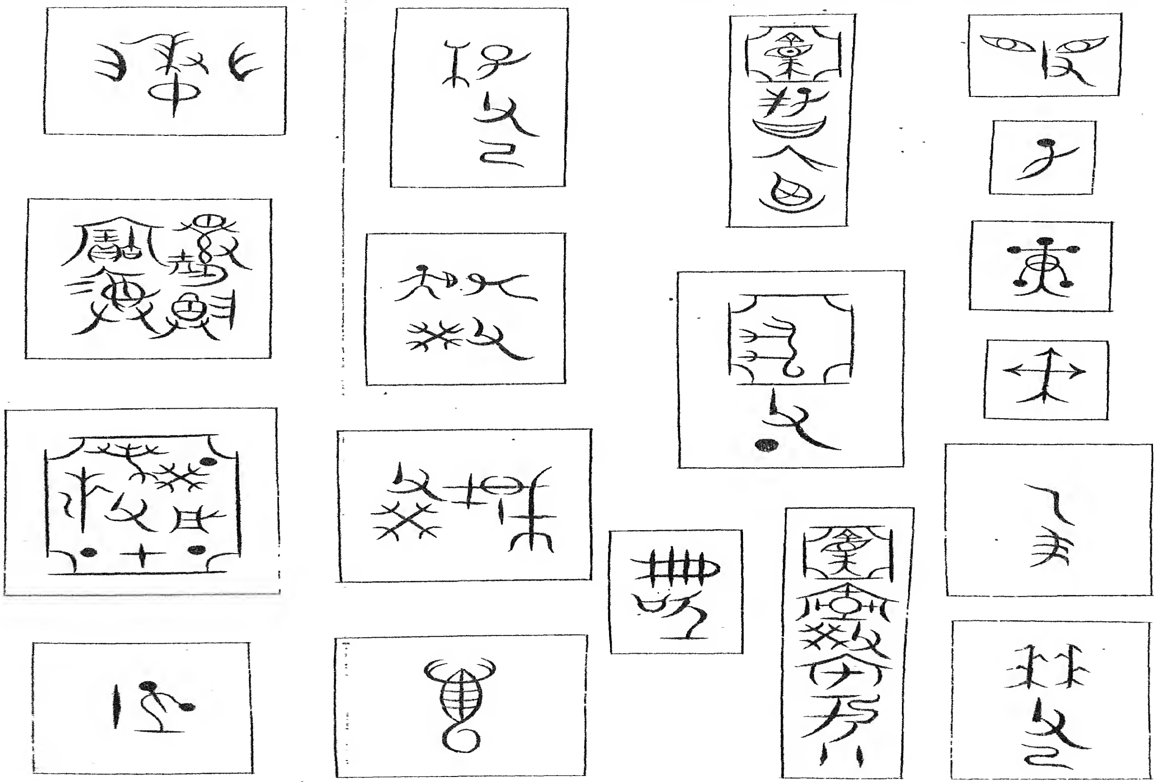


Inscriptions which are thought to be of the Dynasty of the Tcheou.





Inscriptions found upon Ancient Vases which are thought to be of the Dynasty of the Cha



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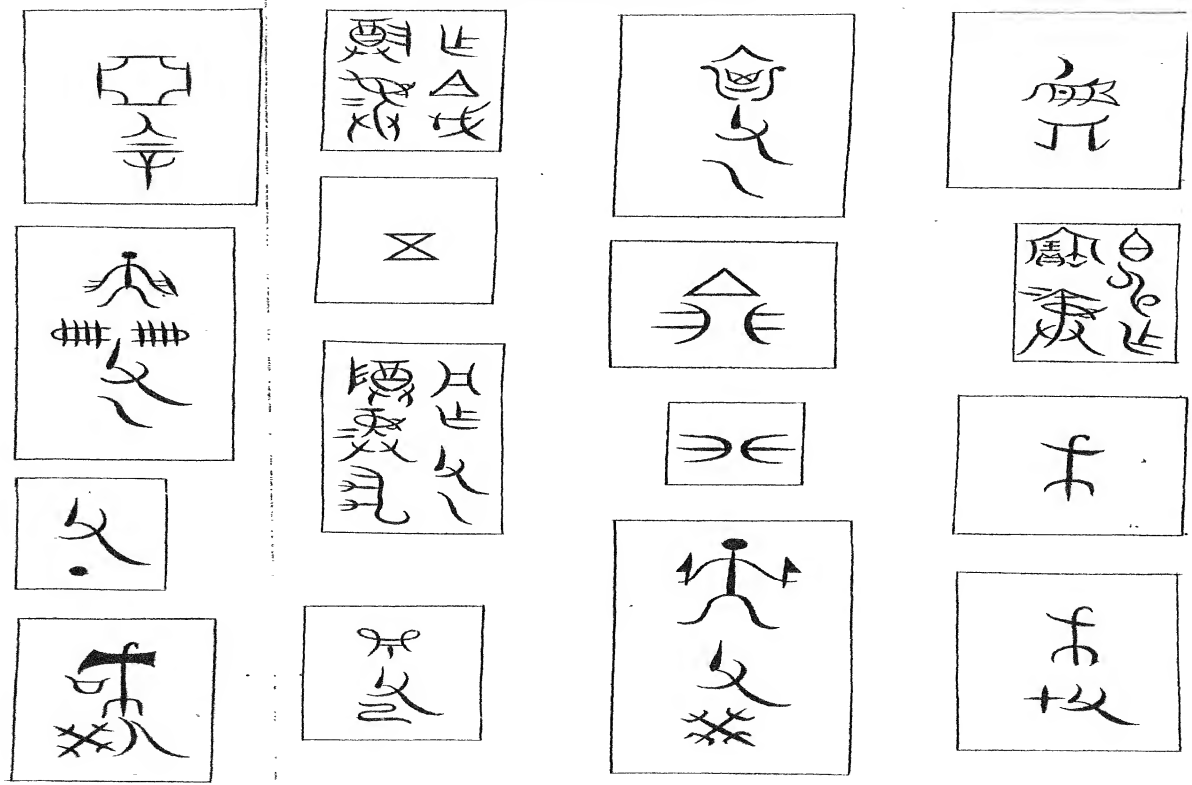

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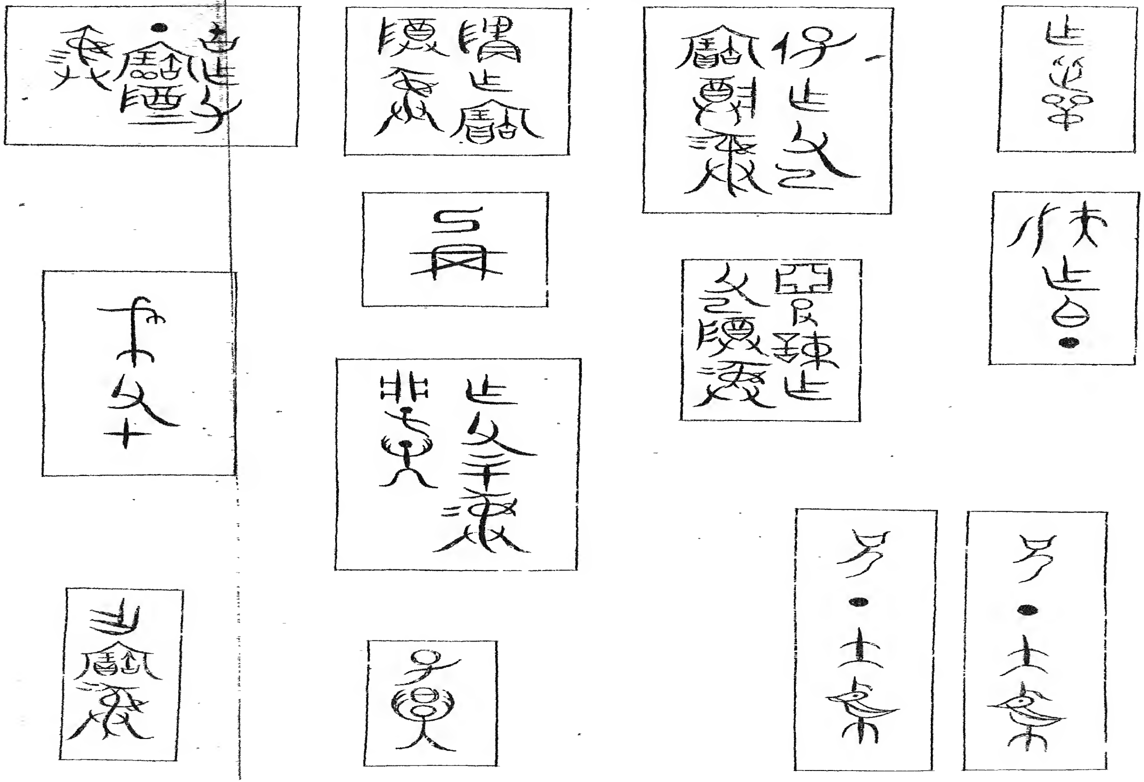

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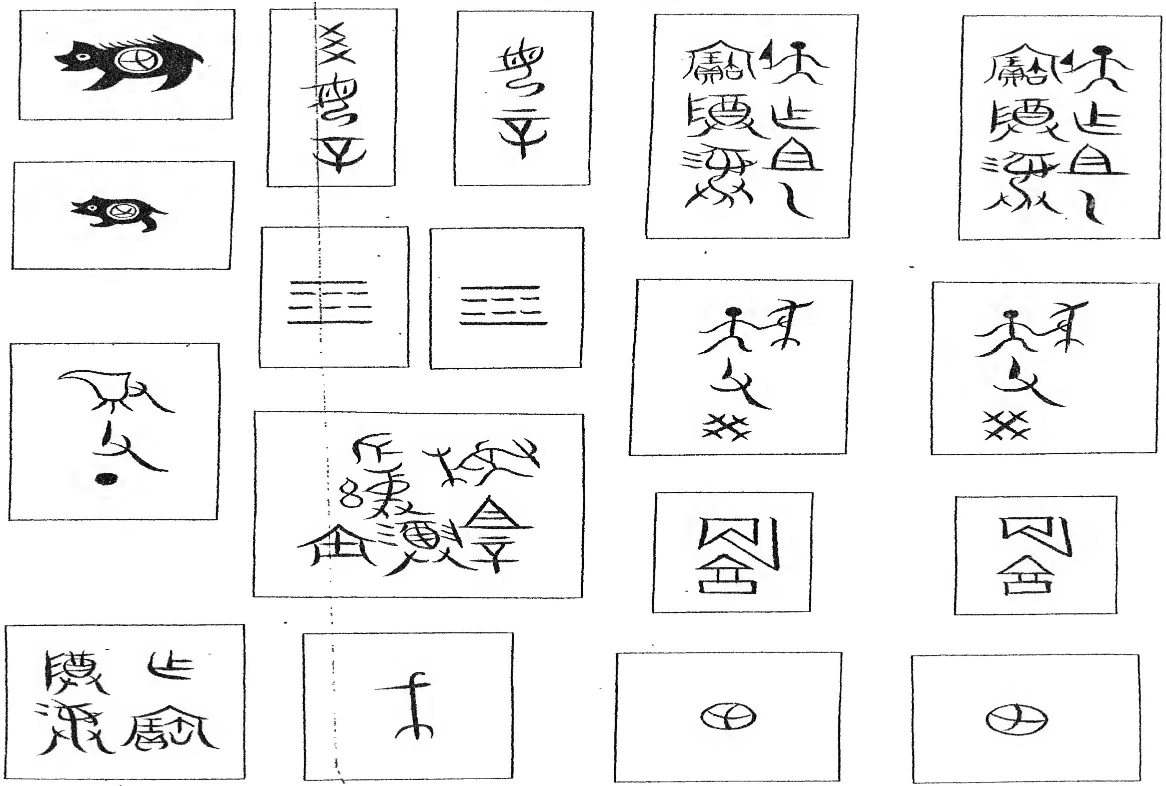

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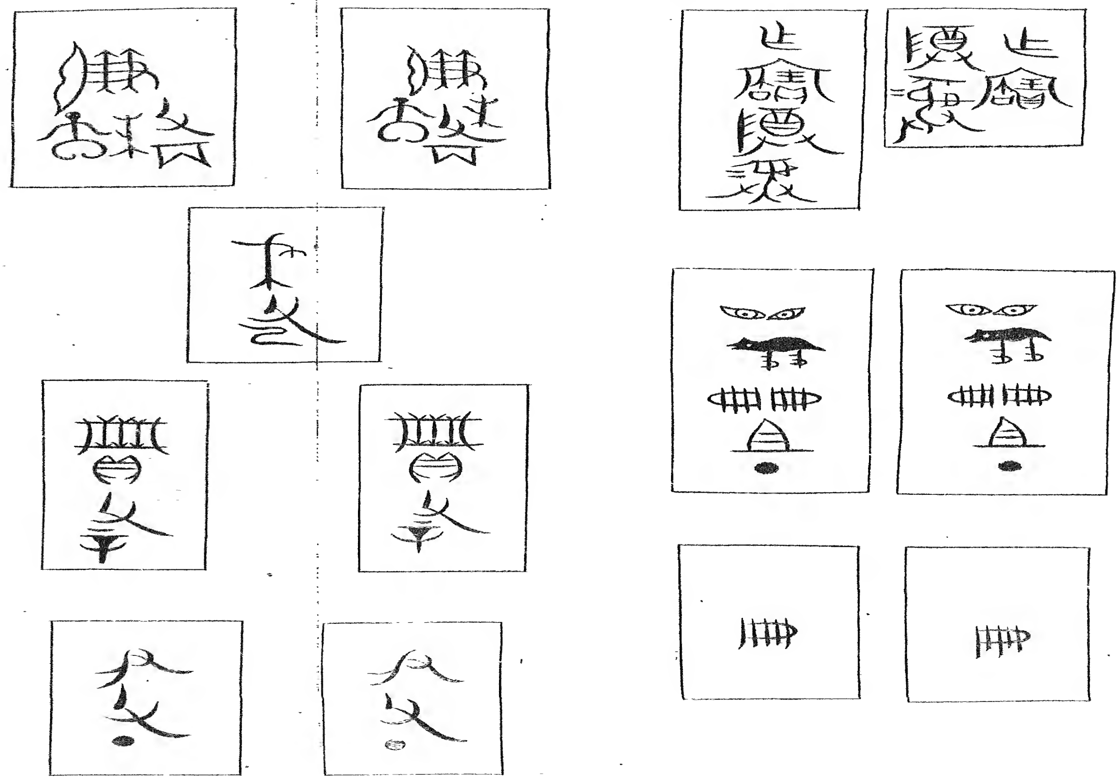

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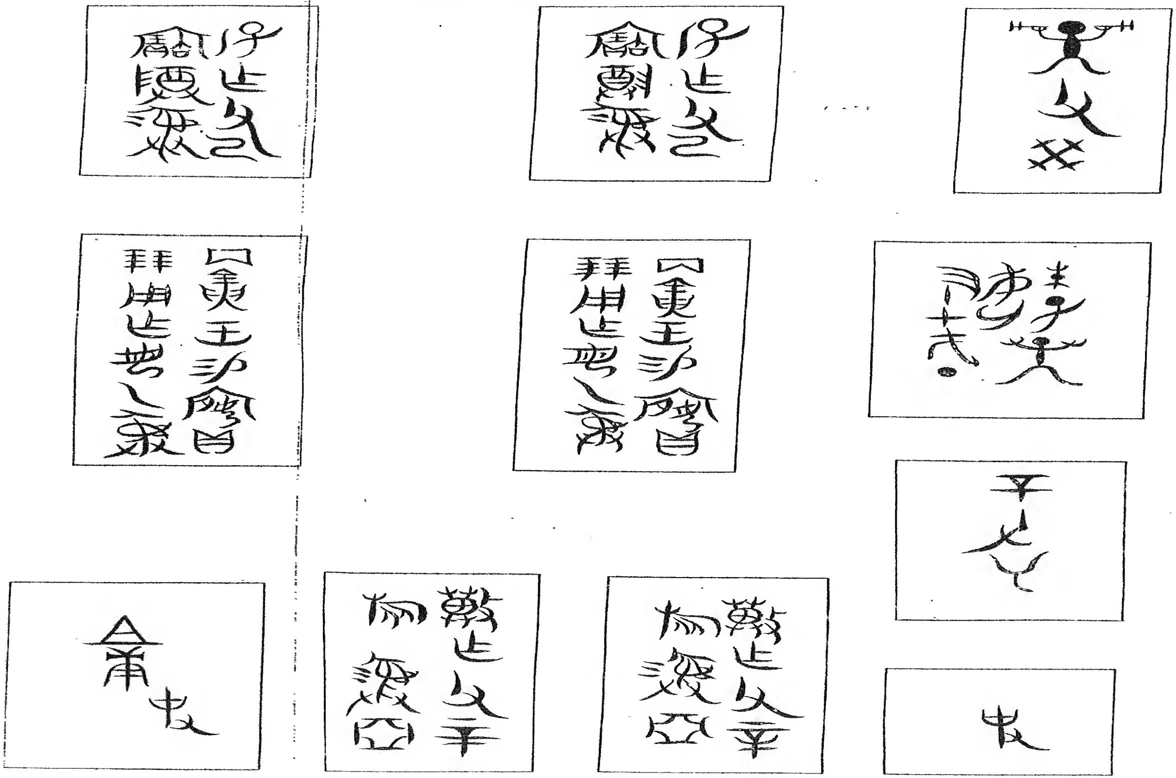
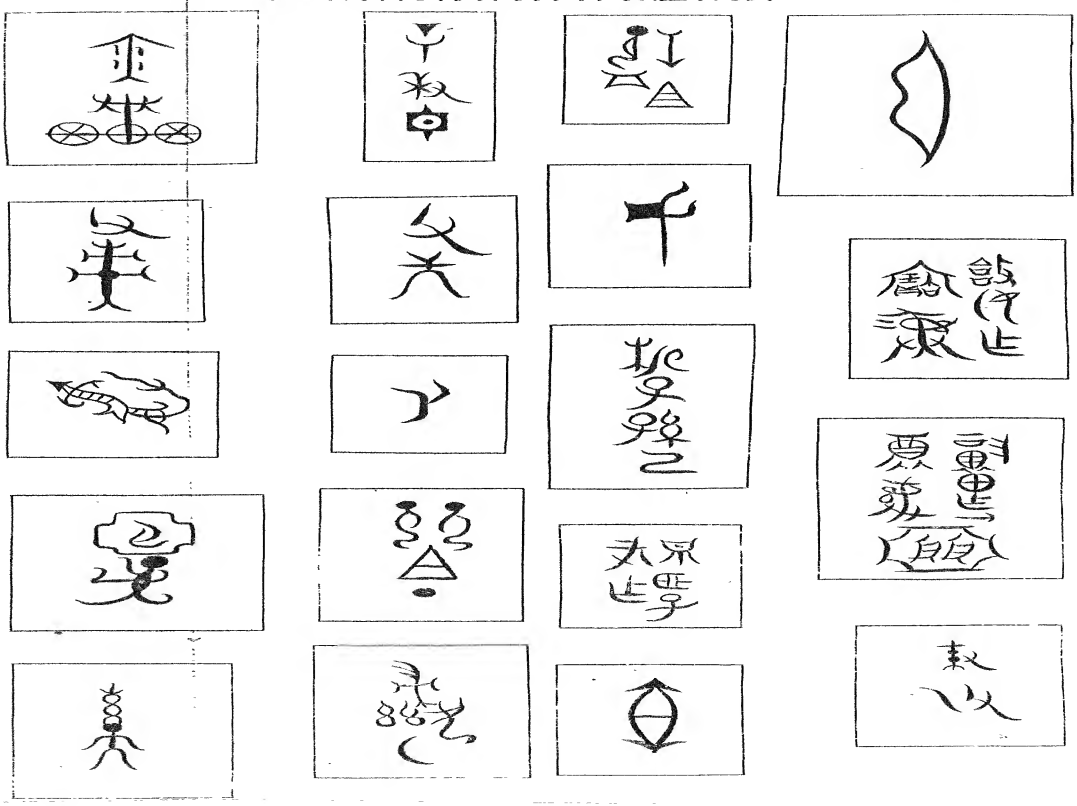


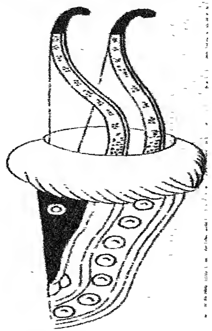
Continuation & End.



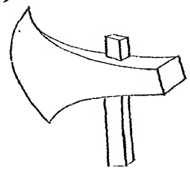
Chinese Antiquities.

Philos. Trans. Vol. LIX. TAB. XL.

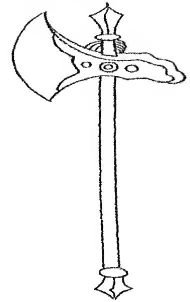
Bows & Case



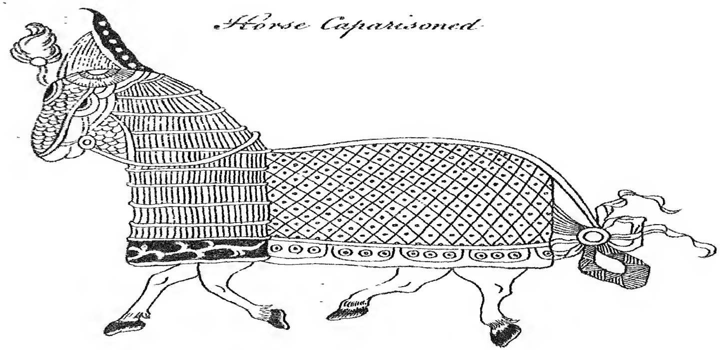
Carpenters Axe



Axe used in War

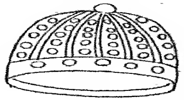


Horse Caparisoned

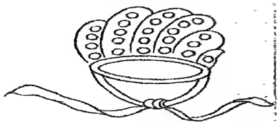


Continuation.

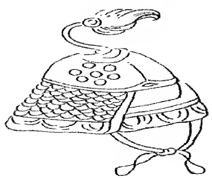
Bonnet



Bonnet

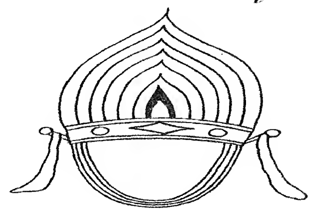


Casquet

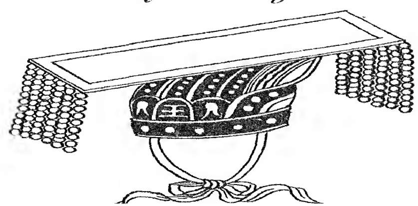


Statue of a Lamb

Bonnet of Sacrifices



Bonnet of Ceremony



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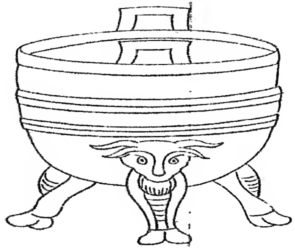
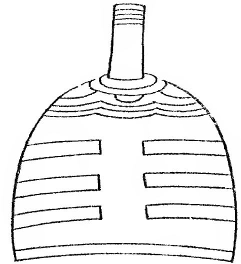
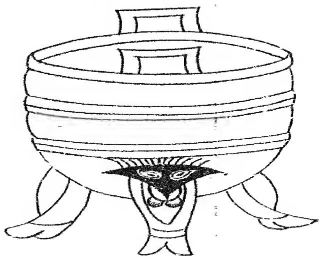
Vessel for the Hall of Ancestors



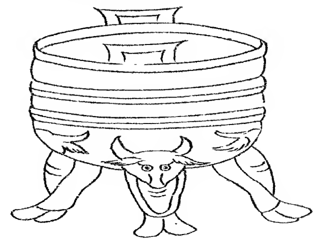
Bell



Vessel for the Hall of Ancestors



Vessel for the Hall of Ancestors



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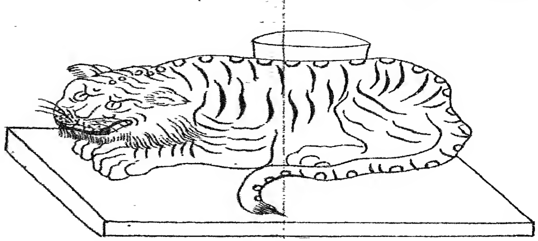
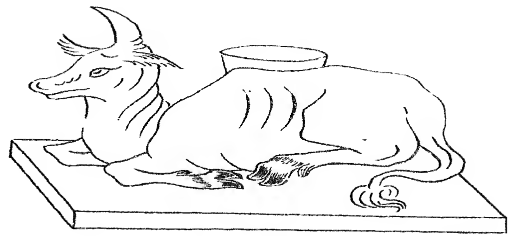
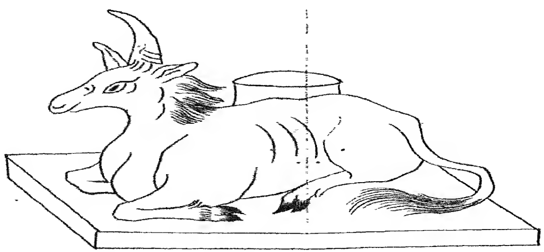
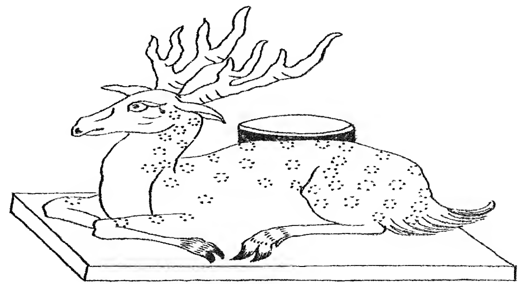
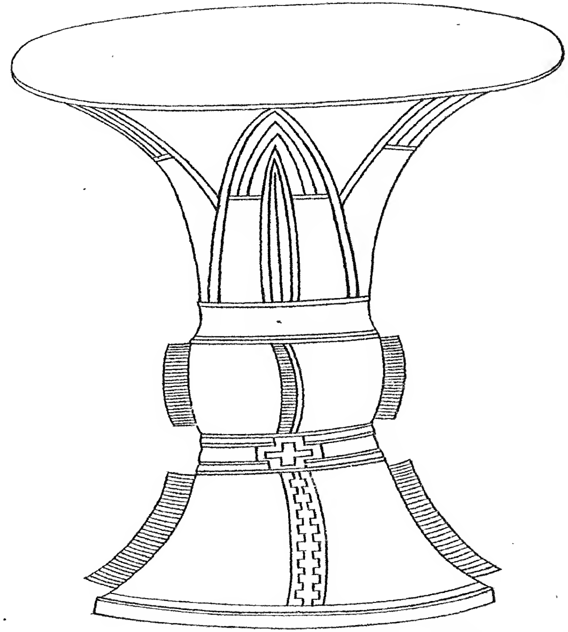








A Vase which is thought to be of the Dynasty of the Chang. The height is 1 Foot, 3 Lines; the Aperture, 5 Inches, 2 Lines; the depth, 7 Inches, 1 Line: French Measure. It Weighs about 2 Pounds.



This Vase, & the two following, are remarkable for the Crosses which one sees clearly traced therein. I have found several others of this form, with Crosses; of which the Chinese say nothing. Nevertheless, as these Vases were for Sacrifices; & as they are the only ones that have Crosses: It is not credible, that this should be pure chance. However, since they have no inscription upon them; I would not warrant their being so Ancient, as the [illegible] Antiquary says they are. Perhaps they mount no higher than to the Han; or, even the Tang.

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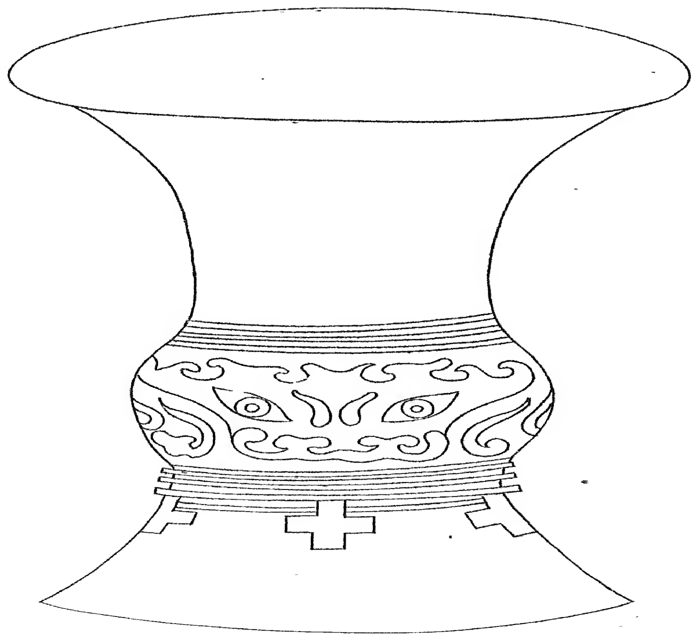
A Vase which is thought to be of the Dynasty of the Tcheou. The height is of 5 Inches, 5 Lines; with an Aperture of 4 Inches, 3 Lines; & the depth 4 Inches, 1 line. It Weighs a little more than 12 Ounces.



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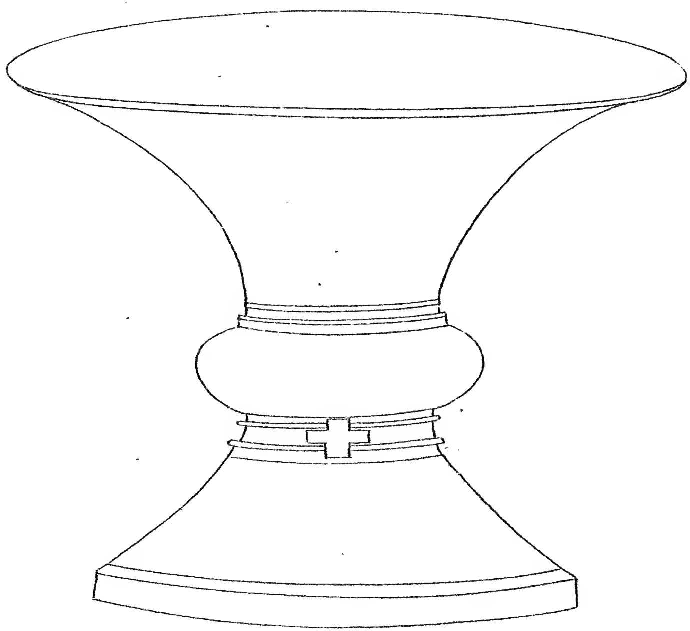
Another Vase of the same Dynasty; in height 6 Inches, 5 Lines; the Aperture 4 Inches 2 Lines: the depth 4 Inches 3 lines: Weight 13 Ounces.



LXVII. *Observation of the Transit of* Mercury *over the* Sun, October 25, 1743. *In a Letter to Professor* Bliss, *late Astronomer Royal and F. R. S. from* John Winthrop, *F. R. S. Professor of Astronomy in* Cambridge, New England.

Rev. Sir,

Read at R. S. Nov. 10, 1763.

GIVE me leave to lay before you an observation of the transit of Mercury, 25 October, 1743; which, if I mistake not, will determine the longitude of Cambridge, New England, with more exactness than any of the observations that have been used for that purpose. I adjusted my clock by correspondent altitudes of the bright star of Aries, taken the night before the transit with a quadrant of two feet radius; and on the day of the transit, by correspondent altitudes of the Sun; all of which agreed within 5″; and allowed for the difference of the Sun's declination, morning and afternoon.

The morning was fair and calm, but hazy; notwithstanding which, I had a good view of the Planet, and with a 24 feet telescope observed that

At $8^h\ 17'\ 5''$ ☿ in his egress touched ☉'s limb.
$18'\ 58''$ went off intirely.

I could

I could not be ſo certain of the moment when he left the Sun, as of his interior contact. For the Sun's limb, undulating in the vapours of the horizon, made it ſomewhat difficult to judge when the indenture, formed by the Planet upon it, intirely ceaſed. However, I believe this latter obſervation may be relied on to 4 or 5″. The compariſon of this obſervation with thoſe made in Europe will, I preſume, determine the difference of meridians within a few ſeconds of time.

I beg, Sir, you will excuſe the freedom I have now taken, ſince it is in the cauſe of aſtronomy, and allow me to ſubſcribe myſelf, with great reſpect,

Reverend Sir,

Your moſt obedient,

humble ſervant,

Cambridge, New England,
20 June, 1763.

John Winthrop.

LXVIII. *A Method*

LXVIII. *A Method of working the Object Glasses of Refracting Telescopes truly spherical. By the late Mr.* James Short, *F.R.S.**

Read Jan. 25, 1770.

PREPARE two plates or tools of brass, the one convex, and the other concave, being both portions of a sphere of the same radius as the focal length of the object glass you want to have, or rather of a radius somewhat longer than the focal length you want, for a dioptrical reason; let these plates or tools be between two and three times the breadth of the object glass desired; or, in long focal lengths, twice the breadth will be sufficient: let these tools be of a sufficient thickness in proportion to their breadth or diameter, and let them be ground with fine emery exactly true to one another, working them alternately, the one above the other, to preserve the same focal length; or, if it is desired longer, you must work the convex above the concave; or, if desired shorter, you must grind the concave above the convex.

After this, you prepare another brass-plate or tool, of the same breadth and thickness as the two former,

* This paper, which was delivered, sealed up, by Mr. Short, at the Society, on the 30th of April, 1752, was, after his death, opened by the Council, and ordered to be printed.

 and

and of the ſame radius of concavity; its being truly turned on a lathe will be ſufficient for this purpoſe; which tool is to ſerve afterwards for the poliſhing of the two ſurfaces of your object glaſs, and therefore called the poliſhing tool.

Prepare a piece of ſtraw-coloured glaſs, of the plate glaſs kind, of the proper diameter for the object glaſs you deſire, which ought always to be broader than the proper aperture for that length; let this piece of glaſs be ground flat, in another tool, on both ſides, and as nearly parallel as may be, and ſomewhat poliſhed, in order to diſcover whether there are any veins or flaws in the glaſs. When you are ſatisfied of the goodneſs of the glaſs, you are then to prepare a handle to faſten your glaſs to. Great care muſt be taken in this, for fear of bending your glaſs by the handle; my method is this; I take a flat piece of braſs, or rather of the concavity of the ſphere, to which the glaſs is to be ground; this piece of braſs ſhould not be thicker than $\frac{2}{3}$ of the thickneſs of the glaſs, of a circular form, leſs in breadth ſomewhat than the glaſs itſelf, and having ſides of the ſame form, at right angles to the flat piece of braſs, and theſe ſides ought to be of ſuch a ſhape as that the fingers may eaſily apply to it in working, and theſe ſides ſhould be as low as may conveniently be, and no thicker than about $\frac{2}{3}$ of the glaſs. This handle is to be faſtened to the glaſs, by warming the glaſs and handle gently before a fire, and laying ſome pitch upon the glaſs thus warmed, till it becomes ſoft like melted wax; and then laying your braſs handle, a little heated, on the pitch, you preſs it a little, till you are ſure there is nothing between the

glaſs

glaſs and handle but pitch; you then lay down the glaſs and handle upon ſomething flat, taking care that the handle is in the middle of the glaſs, till it is entirely cold. It is very material to know, that the pitch, to be uſed for faſtening the handle to the glaſs, muſt be ſoft pitch, that has never been uſed, nor melted; for any other pitch will infallibly bend the glaſs.

You then grind your glaſs in the concave tool with emery, and give it the proper figure and ſmoothing for the laſt poliſh, in the common manner.

In order to give your glaſs the laſt poliſh, which is the moſt difficult part of the whole work, you are to prepare ſome pitch for covering the before mentioned poliſhing concave tool, which is done in this manner: Take ſome pitch, and melt it in an iron ladle, and let it boil for a quarter of an hour or thereabouts; by this boiling, the pitch, when cold, will become hard and brittle; or you may ſhorten this operation, by melting equal quantities of pitch and roſin, and then there is no occaſion to let it boil ſo long. Your pitch being thus prepared, you again melt it, and take it off the fire, and let it ſtand till the pitch becomes pretty cold, or of a thickiſh conſiſtence; and having warmed the poliſhing tool a little, to make the pitch ſtick to it, you pour out of the ladle upon the poliſhing tool as much of the pitch as you judge will cover the whole tool, when ſpread out, to about the thickneſs of $\frac{1}{8}$ of an inch; you then invert this tool with the pitch upon it, and preſs it upon the convex tool, which muſt be quite dry, clean, and cold, in order to give it the figure of the convex tool; in caſe it has not ſpread out ſo as to cover the whole ſurface of

of the poliſhing tool, you warm the pitch by holding it before the fire, and preſſing it upon the convex tool, as before, till it has entirely covered the ſurface of the poliſhing tool; you then plunge it into cold water, till the braſs is quite cold.

N. B. In order to know if your pitch is hard enough, you preſs the edge of the nail of your thumb upon it, and if it receives an impreſſion, the pitch is not hard enough.

You then proceed to prepare this poliſhing tool, for the laſt poliſh of your glaſs, by grinding this poliſhing tool upon the convex tool with pretty coarſe emery, and a ſmall quantity of water, in the common way that tools are ground one upon another; but this muſt be done only for a ſmall ſpace of time, and the poliſhing tool muſt have no other preſſure than its own weight, for fear of ſome of the emery ſticking in the pitch, and you muſt never allow the emery to grow dry; when you have ground the pitch ſo as to be all over of the ſame colour, you then waſh the pitch from all the emery with a bruſh and clean water; after this you take a bottle of water, and holding the pitch tool in a ſloping poſition, you pour water out of the bottle ſo as to fall upon every part of its ſurface.

You then place the poliſhing tool in a horizontal poſition, and you put upon it ſome putty, waſhed from all its gritty particles, but it need not be the fineſt waſhing, and you put a good deal of water upon your poliſhing tool, mixing the putty and it together, and you poliſh your glaſs upon this pitch poliſher in the common manner of poliſhing glaſſes.

After

After you have poliſhed your glaſs about ten minutes, you again grind your poliſher upon the convex tool with emery, as before, for fear the pitch has, by working, loſt any of its proper figure; and the oftener you do this, the truer will be the figure of your glaſs; and in this manner you proceed till the glaſs is quite poliſhed.

You then take your glaſs off its handle, by holding it before the fire, till it is ſo warm that you can ſlide the handle off the glaſs; and whilſt the glaſs is warm, you take off as much of the pitch as you can with the ſharp edge of a knife; you then lay the glaſs down to cool, and, when quite cold, you drop ſome ſpirits of wine upon it; and this, with a cloth, will wipe off the reſt of the pitch.

You then examine the center of the ſurfaces of your glaſs; and if it lies to one ſide of the center of your glaſs, mark that place with a ſpot of ink, and then put on your handle as before, upon the ſide that is now poliſhed, with its center over the ſpot of ink, and grind your glaſs as before, till the circular remaining part of the glaſs to be ground is as much diſtant from the center of the glaſs on the other ſide from the ſpot as the ſpot was from the center of your glaſs; you then by heat return your handle to the center of the glaſs, and proceed to grind and poliſh this ſide of the glaſs as before.

N. B. The concave and convex tools ſhould be ground with fine emery, after you have done one ſide of your glaſs; for the oftener theſe are ground together, you will be the more ſure of having your figure true.

ERRATA.

P. 153, l. 23, *for* knew *read* know.
P. 154, l. 10, *for* $9'' - - 3''$,394 *read* $9'' - - 3'''$,394.
P. 187, l. 24, *for* that the *read* that though the.
l. 27, *for* with reſpect to obſtructing *read* with reſpect to him in obſtructing.
P. 434, l. 21, *for* Eugubini *read* Iguvini.

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